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Change-over designs for factorial experiments with a control treatment

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CHANGE-OVER DESIGNS FOR FACTORIAL EXPERIMENTS WITH A CONTROL TREATMENT

by

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Neofitas Sifa Mvoi

A thesis submitted in partial fulfilment of the requirements for the award of a

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in the University of Wollongong

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Abstract

The need for change-over designs is shown. Models used for change-over designs are provided. The optimality criteria for good designs are given and examples for optimal designs are provided. The factorial structure for factorial change-over designs is defined and the condition necessary for a factorial change-over design to have factorial structure is given. This condition is also applicable when the change-over design is extended to have factorial treatments with a control.

Change-over designs for $2 \times k$ factorial experiments $(k = 2,3,4...)$ and a control treatment are investigated for t treatments, t periods and $2t$ experimental units. Designs which give more precise estimates for the main effects of the 2-level factor than Williams squares designs are given and a method of construction of these designs is provided. A change-over design for a 3 by 3 factorial experiment with a control is given as an example of a design with an even number of treatments. Designs that are best amongst cyclically generated designs are given for t treatments, t periods and t experimental units (where t is odd). Extra period designs are shown to be good for factorial experiments with a control.

The idea of natural contrasts is ruled out for factorial experiments with a control. Good cyclic change-over designs for factorial experiments with a control are given for situations where the number of periods, p, is less than the number of treatments, t. Optimal designs for some situations in which p is less than t are provided and the nature of their reduced coefficient matrices for estimating direct effects and residual effects separately is given. Optimal replication is discussed and an example is provided of designs that have optimal replication for the estimation of the main effects of the factors.

Table of Contents

List of Tables

Chapter 1
Introduction

A brief introduction to the topic is given. A review of the previous work is provided. The model that is adopted is specified. The optimality criterion is given and two methods that provide optimal designs are mentioned.

1.1 *Brief Introduction*

Consider an experiment in which t treatments are applied to n experimental units in p successive time periods such that each experimental unit receives a treatment in each of the time periods. In each period, apart from the first, an observation made on an experimental unit is affected by the treatment applied at the present time as well as the residual effects of treatments applied on the same unit at previous time periods. The allocation of treatments of such an experiment follows a change-over or cross-over design. The residual effect attributed to the treatment applied in the preceding period on an experimental unit for each of the time periods $2, 3, \ldots, p$ is called the first-order residual effect. Higher order residual effects are considered negligible in most experiments.

There are several ways in which the usage of change-over designs can be justified. Hedayat and Afsarinejad (1975) have listed a few of these ways as follows:-

- An experimenter may be forced to use each experimental unit several times by limitations on the budget.
- In some experiments, the treatment effects may not have serious damaging effect on the experimental units resulting in the experimental units being used in successive experiments.

- The experimental units may be humans or animals and the nature of the experiment may be such that it calls for special training over a long period of time. Time limitation may force the experimenter to use these units for several tests.
- One of the objectives of the experiment may be to find out the effect of different sequences as in drug, nutrition or learning experiments.
- The experimental units may be scarce leading to repeated tests on the ones available.

The elimination of between subject variability is another important justification of change-over designs (suggested by a referee).

Change-over designs have a wide range of applicability. They have been used in many branches of scientific inquiry such as agriculture, animal husbandry, biology, education, food science, market research, medicine, pharmacology, social sciences and engineering. Depending on the nature of the experiment, the primary interest may be the estimation of the direct treatment effects or first order residual effects.

Example 1

This example is quoted from Russell (1991). In a wine-tasting experiment, judges are supposed to give their impression on some ordinal scale, of several brands of wine. The time interval between tests for each judge is the same. A change-over design is most appropriate as a judge's impression of a wine is influenced by his or her impression of the wine tasted immediately beforehand.

Example 2

This example is taken from Patterson (1950). In a dairy farming experiment, the effect of different rations on milk production is sought. Eighteen cows are fed, for five consecutive week periods, three rations, namely good hay, poor hay and straw. The cows are divided into six groups of three cows each, each group receiving a different ration in each time

period. The response recorded was the yield of milk for each five-week period. There are no residual effects in the first period. In subsequent periods, the yield of milk is expected to be affected directly by the kind of ration the cows are given in that period as well as the residual effect of the ration given to the cows in the preceding period. A change-over design is thus the most appropriate in this situation.

Methods of construction of change-over designs have been made available by several workers. Williams (1949) provides a method of construction for t treatments, t periods and for t or $2t$ experimental units depending on whether t is even or odd. Davis and Hall (1969) have derived change-over designs from cyclic incomplete block designs, subsequently referred to as cyclic change-over designs. The cyclic change-over designs in t treatments, p periods and $n=bt$ experimental units arranged in b blocks, are obtained from cyclic development from the b generating sequences. The cyclic nature of these designs simplifies the computations on these designs. Cheng and Wu (1980) have provided a method of construction for strongly balanced uniform change-over designs for t treatments, p periods and n experimental units, where t^2 is a divisor of n and p/t is an even integer. Russell (1991) has given the construction of change-over designs when there are fewer units than treatments.

Work has also been done on change-over designs for factorial experiments. Fletcher and John (1985) have shown that the importance of factorial effects or treatment contrasts should be considered in the construction of change-over designs for factorial experiments. They have also defined the property of factorial structure which is based on the orthogonality of the different factorial effects both direct and residual. Fletcher (1987) has generalised the results on cyclic change-over designs and used the concept of the factorial structure to provide change-over designs for factorial experiments. He provides a simplified way of determining the direct and residual canonical efficiency factors for generalised cyclic change-over designs

which he inherently uses to select good designs. Lewis, Fletcher and Matthews (1988) have made available efficient designs involving three or four periods and two treatment factors when both factors have two levels or one has two levels and the other has three levels. Their designs take the form of generalised cyclic change-over designs and have used the concept of natural contrasts or bricks to construct them. Yates and Lewis (1995) have provided designs for factorial experiments for specified treatment contrasts. The designs by Yates and Lewis are efficient with or without residual effects.

The objective of this work is to consider change-over designs for factorial experiments which have a control as an additional treatment.

1.2 Model

The model that is adopted for this work assumes that there is no correlation between different observations. The residual effect received by an experimental unit is assumed to be a fixed effect. The treatments are labelled ***0, 1, 2, 3,..., t-1.*** For a design ***d, d(i,j)*** denotes the treatment applied to the jth experimental unit in the ith time period. The observation made on the jth experimental unit in the ith time period is given by

$$Y_{ij} = \alpha_i + \beta_j + \tau_{d(i,j)} + \rho_{d(i-1,j)} + e_{ij} \qquad (1.2.1)$$

(i=1, 2, 3,..., p; j=1, 2, 3..., n; d(i,j) ∈ {0, 1, 2, 3,..., t-1})

where α_i, β_j, $\tau_{d(i,j)}$ and $\rho_{d(i-1,j)}$ are respectively the ith period effect, jth unit effect, the direct treatment effect and residual treatment effect experienced by the jth unit at the ith time period. The e_{ij}s are the error terms, assumed to be uncorrelated with mean 0 and variance σ^2.

Other models have been used for change-over designs. Kunert (1985) considered a change-over design in which the errors on the same unit are correlated and the correlation decreases exponentially with time. In his model he assumed that the measurements occur at equidistant periods in time. He went further to show that the optimal designs for his model are similar to the optimal designs of model (1.2.1).

Another model that has been used takes into account the interaction between direct effects and first-order residual effects. Patterson (1970) has listed the most efficient designs for estimating direct × first-order residual interaction. Kok and Patterson (1976) have listed conditions under which first-order residual effects are orthogonal to direct × first-order residual interaction for factorial designs in which direct effects are orthogonal to first-order residual effects.

1.3 Optimality criteria

Model (1.2.1) can be written in the following matrix form

$$Y = X_1\tau + X_2\rho + X_3\alpha + X_4\beta + E \qquad (1.3.1)$$

where Y is an np by 1 vector of observations, τ, ρ, α and β are vectors of parameters. X_1, X_2, X_3 and X_4 are matrices of 0's and 1's corresponding to these parameters. E is the vector of random errors. Equation (1.3.1) can be rewritten in the form

$$Y = X\theta + E \qquad (1.3.2)$$

where $X = [X_1, X_2, X_3, X_4]$ and $\theta = (\tau^T, \rho^T, \alpha^T, \beta^T)^T$.

An estimator of θ can be obtained by the least squares method which yields the normal equations $X^T X\hat{\theta} = X^T Y$ where $\hat{\theta}$ is a non-unique estimator of θ. Estimators of τ and ρ

are obtained from the reduced normal equations which are obtained by eliminating α and β from the normal equations given above.

Suppose for a design $d \in D$, where D is a given class of designs with t treatments, C_d is the reduced information matrix for the estimation of treatment effects. Then suppose the class $C = \{C_d, d \in D\}$ of matrices contains a C_{d*} for which

(a) $C_{d*} = aI_t + bJ_{t,t}$ where I_t is the identity matrix of dimension t, and $J_{t,t}$ is a t by t matrix of ones

(b) $trC_{d*} = \max_{d \in D} trC_d$

Kiefer (1975) has shown that d^* is **universally optimal** in D. A matrix having the form given in (a) is said to be **completely symmetric.**

For change-over designs, the matrix $M=((m_{ij}))$ of dimensions t by t, where m_{ij} is the number of times the ith treatment is immediately preceded by the jth treatment, is of particular importance. Using Kiefer's tool of optimality, Hedayat and Afsarinejad (1978) have shown that a change-over design for which there is equal replication within periods as well as equal replication within experimental units is universally optimal for the estimation of direct effects as well as first-order residual effects if the matrix M is an integer multiple of $J_{t,t} - I_t$.

1.4 Optimal designs

Williams (1949) designs, mentioned earlier, are universally optimal designs. When t is even, the design is provided by a special kind of latin square which results in each treatment following every other treatment once. When t is odd, the design is provided by a special kind of latin square and a second square formed by reversing the order of the rows of the

first square. In this case, each treatment follows every other treatment twice. Tables 1.1 and 1.2 give examples of Williams squares and their M matrices.

Table 1.1: A change-over design with t=p=n=6

	Experimental Unit					
Period	1	2	3	4	5	6
1	0	1	2	3	4	5
2	5	0	1	2	3	4
3	1	2	3	4	5	0
4	4	5	0	1	2	3
5	2	3	4	5	0	1
6	3	4	5	0	1	2

Matrix M for the design:

$$M = \begin{bmatrix} 0 & 1 & 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 & 1 & 1 \\ 1 & 1 & 0 & 1 & 1 & 1 \\ 1 & 1 & 1 & 0 & 1 & 1 \\ 1 & 1 & 1 & 1 & 0 & 1 \\ 1 & 1 & 1 & 1 & 1 & 0 \end{bmatrix}$$

Table 1.2: A change-over design for t=p=5, n=2t=10

	Experimental Unit									
Period	1	2	3	4	5	6	7	8	9	10
1	0	1	2	3	4	2	3	4	0	1
2	4	0	1	2	3	3	4	0	1	2
3	1	2	3	4	0	1	2	3	4	0
4	3	4	0	1	2	4	0	1	2	3
5	2	3	4	0	1	0	1	2	3	4

Matrix M for the design:

$$M = \begin{bmatrix} 0 & 2 & 2 & 2 & 2 \\ 2 & 0 & 2 & 2 & 2 \\ 2 & 2 & 0 & 2 & 2 \\ 2 & 2 & 2 & 0 & 2 \\ 2 & 2 & 2 & 2 & 0 \end{bmatrix}$$

In the case where the number of experimental units n is less than t, then the off-diagonal elements of M cannot be all equal. Russell (1991) has provided a method of obtaining good designs for $n < t$. When t is even, a design is obtained by selecting n columns from a Williams square so as to minimise the average variances of the pairwise contrasts of the direct and residual effects of the resulting design. The resulting design has an M matrix which has off-diagonal elements as similar as possible. When t is odd, Russell provided a method of constructing a latin square in which the off-diagonal elements of M are as similar as possible. The design for $n < t$ when t is odd is then the choice of n columns from this square such that the resulting design is optimal. Table 1.3 gives an example of a Russell square and the resulting M matrix.

Table 1.3: Russell square for t=9

0	1	2	3	4	5	6	7	8
5	6	7	8	0	1	2	3	4
6	7	8	0	1	2	3	4	5
4	5	6	7	8	0	1	2	3
7	8	0	1	2	3	4	5	6
2	3	4	5	6	7	8	0	1
8	0	1	2	3	4	5	6	7
1	2	3	4	5	6	7	8	0
3	4	5	6	7	8	0	1	2

Matrix M is as follows:

$$M = \begin{bmatrix} 0 & 0 & 1 & 1 & 1 & 1 & 1 & 2 & 1 \\ 1 & 0 & 0 & 1 & 1 & 1 & 1 & 1 & 2 \\ 2 & 1 & 0 & 0 & 1 & 1 & 1 & 1 & 1 \\ 1 & 2 & 1 & 0 & 0 & 1 & 1 & 1 & 1 \\ 1 & 1 & 2 & 1 & 0 & 0 & 1 & 1 & 1 \\ 1 & 1 & 1 & 2 & 1 & 0 & 0 & 1 & 1 \\ 1 & 1 & 1 & 1 & 2 & 1 & 0 & 0 & 1 \\ 1 & 1 & 1 & 1 & 1 & 2 & 1 & 0 & 0 \\ 0 & 1 & 1 & 1 & 1 & 1 & 2 & 1 & 0 \end{bmatrix}$$

1.5 Some relevant definitions

Proper matrix

A square matrix with all row sums and column sums equal is called a **proper matrix**.

Kronecker product

Let $X = \left(\left(x_{ij}\right)\right)$ be a r by s matrix and let $Y = \left(\left(y_{ij}\right)\right)$ be a u by v matrix. The **Kronecker product** of X and Y, denoted by $X \otimes Y$, is the ru by sv matrix defined as

$$X \otimes Y = \begin{bmatrix} x_{11}Y & x_{12}Y & \cdots & x_{1s}Y \\ x_{21}Y & x_{22}Y & \cdots & x_{2s}Y \\ \vdots & \vdots & \vdots & \vdots \\ x_{r1}Y & x_{r2}Y & \cdots & x_{rs}Y \end{bmatrix}$$

Estimability

Let $\hat{Z}$ be a least square estimator of the vector of parameters Z such that

$$E(\hat{Z}) = \Omega C Z$$

$$V(\hat{Z}) = \Omega C \Omega Z \qquad (1.5.1)$$

where Ω is a generalised inverse of the reduced information matrix C for estimating the parameters Z.

A linear function $a^T Z$ is said to be **estimable** if there exists some linear combination of responses $b^T Y$ such that $E(b^T Y) = a^T Z$. Since $\hat{Z}$ is a linear function of the responses it follows from (1.5.1) that $a^T Z$ is estimable if the coefficient a is chosen to satisfy $a^T = a^T \Omega C$.

This is the **estimability** condition.

Again from (1.5.1) it follows that

$$Var(a\hat{Z}) = a^T \Omega a \sigma^2 \qquad (1.5.2)$$

Connectedness

Any block design is said to be **disconnected** if the blocks can be split into groups in such a way that the treatments in any one group of blocks are distinct from treatments in other groups. A design is **connected** if it is not disconnected. In a connected design every treatment contrast is estimable from comparisons within blocks. In factorial change-over designs, a design is connected when all contrasts in both direct and residual effects are estimable.

Orthogonality

In the linear model $Y = W_1\alpha_1 + W_2\alpha_2 + \varepsilon$ the parameters α_1 and α_2 are said to be **orthogonal** to each other if their least square estimators are independent, in the sense that the estimator of α_i in this model is the same as the one obtained from the model $Y = W_i\alpha_i$ (i =*1, 2*). That is, the presence and absence of one of the parameter vectors in the model does not affect the estimator of the other. In change-over designs, the estimation of direct effects is orthogonal to period and unit effects if there is equal replication of treatments in units as well as equal replication of treatments in periods.

Chapter 2
Estimation of Direct and Residual Effects and the Factorial Structure

Change-over designs will be considered for factorial experiments. The form of the reduced normal equations for estimating the direct treatment effects and residual treatment effects is worked out. The factorial structure for change-over designs is defined.

2.1 Factorial change-over designs

A factorial experiment of two factors each of two levels is considered. The treatment combinations are to be applied in each of 8 experimental units during 4 consecutive time-periods. First-order residual effects are assumed to be present. The treatments are labelled as a_1b_1, a_2b_1, a_1b_2, a_2b_2. The experiment may be carried out as per the following two designs.

Table 2.1: Design 1

	Subjects							
Periods	1	2	3	4	5	6	7	8
1	a_1b_1	a_2b_1	a_1b_2	a_2b_2	a_1b_1	a_2b_1	a_1b_2	a_2b_2
2	a_2b_1	a_1b_2	a_2b_2	a_1b_1	a_2b_1	a_1b_2	a_2b_2	a_1b_1
3	a_2b_2	a_1b_1	a_2b_1	a_1b_2	a_2b_2	a_1b_1	a_2b_1	a_1b_2
4	a_1b_2	a_2b_2	a_1b_1	a_2b_1	a_1b_2	a_2b_2	a_1b_1	a_2b_1

Table 2.2: Design 2

	Subjects							
Periods	1	2	3	4	5	6	7	8
1	a_1b_1	a_1b_2	a_2b_2	a_2b_1	a_1b_2	a_2b_2	a_2b_1	a_1b_1
2	a_2b_1	a_1b_1	a_1b_2	a_2b_2	a_2b_2	a_2b_1	a_1b_1	a_1b_2
3	a_2b_2	a_2b_1	a_1b_1	a_1b_2	a_2b_1	a_1b_1	a_1b_2	a_2b_2
4	a_1b_2	a_2b_2	a_2b_1	a_1b_1	a_1b_1	a_1b_2	a_2b_2	a_2b_1

3 0009 03143364 7

Design 1 is derived from Williams squares and each treatment follows every other treatment twice. In design 2, each level of factor A follows itself and every other level of factor A an equal number of times. A summary of how different levels of the factors follow each other is given below. The notation $x \rightarrow y$ stands for "x follows y".

Relationship	Design 1	Design 2
$a_1 \rightarrow a_1$	4 times	6 times
$a_1 \rightarrow a_2$	8 times	6 times
$a_2 \rightarrow a_1$	8 times	6 times
$a_2 \rightarrow a_2$	4 times	6 times
$b_1 \rightarrow b_1$	4 times	6 times
$b_1 \rightarrow b_2$	8 times	6 times
$b_2 \rightarrow b_1$	8 times	6 times
$b_2 \rightarrow b_2$	4 times	6 times

The fact that each level of factor A follows itself and every other level of factor A an equal number of times in design 2 implies that the main effects (both direct and residual) can be estimated with greater precision using design 2 rather than design 1. The same applies for factor B. The estimation of interaction effects is expected to be at a greater precision in design 1 than in design 2 since each treatment follows every other treatment the same number of times in design 1.

2.2 Estimation of direct and residual effects

The model adopted for a change-over design is as defined in Chapter 1. Using notation and form given in Cheng and Wu (1980), the following definitions are made:-

n_{iu} = number of times treatment i appears on unit u

$\tilde{n}_{iu}$ = number of appearances of treatment i on unit u in the first $p-1$ periods

l_{ik} = number of appearances of treatment i in period k

m_{ij} = number of appearances of treatment i immediately preceded by treatment j on the same unit.

where $i,j = 0, 1, 2,..., t-1$; $u = 1, 2,..., n$; $k = 1, 2,..., p$.

It follows that

$$\sum_{u=1}^{n} n_{iu} = \sum_{k=1}^{p} l_{ik} = r_i \text{ = number of appearances of treatment } i \text{ in the design}$$

$$\sum_{u=1}^{n} \tilde{n}_{iu} = \sum_{k=1}^{p-1} l_{ik} = \sum_{j=1}^{t} m_{ji} = \tilde{r}_i \text{ = number of appearances of treatment } i \text{ in the first } p-1 \tag{2.2.1}$$

periods.

Also,

$$\sum_{i=1}^{t} n_{iu} = p, \sum_{i=1}^{t} \tilde{n}_{iu} = p-1, \sum_{i=1}^{t} l_{ik} = n$$

$$\sum_{i=1}^{t} r_i = np, \sum_{i=1}^{t} \tilde{r}_i = n(p-1) \tag{2.2.2}$$

$$\sum_{k=2}^{p} l_{ik} = \sum_{j=1}^{t} m_{ij} = s_i \text{ = number of appearances of treatment } i \text{ in the last } p-1 \text{ periods.}$$

The form of $X^T X$ in section 1.3 of the previous chapter is then given by

$$X^T X = \begin{bmatrix} D & M & N_p & N_u \\ M^T & \tilde{D} & \tilde{N}_p & \tilde{N}_u \\ N_p^T & \tilde{N}_p^T & nI_p & J_{p,n} \\ N_u^T & \tilde{N}_u^T & J_{n,p} & pI_n \end{bmatrix} \tag{2.2.3}$$

$D = diag(r_1, r_2, ..., r_t)$, $\tilde{D} = diag(\tilde{r}_1, \tilde{r}_2, ..., \tilde{r}_t)$, $M = \left((m_{ij})\right)$, $N_p = \left((l_{ik})\right)$,

$\tilde{N}_p = \left((\tilde{l}_{ik})\right)$ where $\tilde{l}_{i1} = 0$ and $\tilde{l}_{ik} = l_{i,k-1}$ for $k \geq 2$

$N_u = \left((n_{iu})\right)$ and $\tilde{N}_u = \left((\tilde{n}_{iu})\right)$

The information matrix, derived from (2.2.3), for estimating direct and residual effects jointly is

$$\begin{bmatrix} D & M \\ M^T & \tilde{D} \end{bmatrix} - \begin{bmatrix} N_p & N_u \\ \tilde{N}_p & \tilde{N}_u \end{bmatrix} \begin{bmatrix} nI_p & J_{p,n} \\ J_{n,p} & pI_n \end{bmatrix}^{-} \begin{bmatrix} N_p^T & \tilde{N}_p^T \\ N_u^T & \tilde{N}_u^T \end{bmatrix} = \begin{bmatrix} C_{11} & C_{12} \\ C_{21} & C_{22} \end{bmatrix} = C \qquad (2.2.4)$$

where

$C_{11} = D - n^{-1}N_pN_p^T - p^{-1}N_uN_u^T + (np)^{-1}N_uJ_{n,n}N_u^T$

$C_{12} = C_{21}^T = M - n^{-1}N_p\tilde{N}_p^T - p^{-1}N_u\tilde{N}_u^T + (np)^{-1}N_uJ_{n,n}\tilde{N}_u^T$

$C_{22} = \tilde{D} - n^{-1}\tilde{N}_p\tilde{N}_p^T - p^{-1}\tilde{N}_u\tilde{N}_u^T + (np)^{-1}\tilde{N}_uJ_{n,n}\tilde{N}_u^T$

and H^- denotes a generalized inverse of H satisfying $HH^-H = H$. The reduced normal equations for direct and residual effects are

$$C\hat{Z} = q \qquad (2.2.5)$$

where $\hat{Z} = \begin{bmatrix} \hat{\tau} \\ \hat{\rho} \end{bmatrix}$, $q = \begin{bmatrix} q_1 \\ q_2 \end{bmatrix}$,

and $q_1 = X_1^TFY$, $q_2 = X_2^TFY$ where $F = F_pF_n$, $F_p = I_{np} - \left(1/n\right)X_3X_3^T$ and

$F_n = I_{np} - \left(1/p\right)X_4X_4^T$.

X_1, X_2, X_3, X_4 are as given in (1.3.1) and C is as given in (2.2.4.)

From the relations (2.2.1) and (2.2.2), the ith row sums of $n^{-1}N_pN_p^T$, $p^{-1}N_uN_u^T$, and $n^{-1}p^{-1}N_uJ_{n,n}N_u^T$ are all equal to r_i. Also, the ith row sums of M and $n^{-1}N_p\tilde{N}_p^T$ are both equal to s_i and the ith row sums of $p^{-1}N_u\tilde{N}_u^T$ and $n^{-1}p^{-1}N_uJ_{n,n}\tilde{N}_u^T$ are both equal to $p^{-1}(p-1)r_i$. The jth column sums of M, $n^{-1}N_p\tilde{N}_p^T$, $p^{-1}N_u\tilde{N}_u^T$ and $n^{-1}p^{-1}N_uJ_{n,n}\tilde{N}_u^T$ are

all equal to $\tilde{r}_j$. The ith row sum of $n^{-1}\tilde{N}_p\tilde{N}_p^T$ is $\tilde{r}_i$; for $p^{-1}\tilde{N}_u\tilde{N}_u^T$ and $n^{-1}p^{-1}\tilde{N}_uJ_{n,n}\tilde{N}_u^T$ the ith row sums are both equal to $p^{-1}(p-1)\tilde{r}_i$. These results indicate that

$$C_{ij}1_t = 0 \qquad (2.2.6)$$

for i,j = 1,2 where 1_t is a vector of dimension t of ones.

This implies that Rank (C) $\leq 2t$ - 2. The equality will hold when all contrasts in both direct and residual effects are estimable (Searle 1971). The design will then be connected. A solution of the reduced normal equations (2.2.5) is then given by

$$\hat{Z} = C^{-}q \qquad (2.2.7)$$

2.2.1 Designs with equal replication on both units and periods

The results that follow and subsequent definition of the factorial structure are as given by Fletcher and John (1985).

Let A be a (t-1) by t matrix whose rows represent orthonormal contrasts corresponding to main effects and interactions of a factorial experiment. Since A is orthonormal, $AA^T = I_{t-1}$, and $A^TA = I_t - g_t$ where $g_t = \left(\frac{1}{t}\right)J_{t,t}$ [see Mukerjee(1979)].

Example

In the 2×2 factorial experiment in section 2.1 the orthonormal contrasts chosen are given by the following matrix

$$A = \begin{pmatrix} -0.5 & 0.5 & -0.5 & 0.5 \\ -0.5 & -0.5 & 0.5 & 0.5 \\ 0.5 & -0.5 & -0.5 & 0.5 \end{pmatrix}$$

It can be seen that $AA^T = I_3$ and $A^TA = I_4 - 0.25J_{4,4}$

Assuming connectedness in the design, these contrasts of the direct effects and residual effects can be estimated by

$$(I_2 \otimes A)\hat{Z} = (I_2 \otimes A)\Omega q \qquad (2.2.1.1)$$

Taking the variance of equation (2.2.1.1) results in the covariance matrix for $(I_2 \otimes A)\hat{Z}$;

$$V\sigma^2 = (I_2 \otimes A)\Omega\left(I_2 \otimes A^T\right)\sigma^2 \qquad (2.2.1.2)$$

The classification of the factorial change-over designs is based on certain forms of the matrix V. Matrix V is non-singular and its inverse is

$$V^{-1} = (I_2 \otimes A)C\left(I_2 \otimes A^T\right) \qquad (2.2.1.3)$$

Proof

$$(I_2 \otimes A)C\left(I_2 \otimes A^T\right)(I_2 \otimes A)\Omega\left(I_2 \otimes A^T\right) = (I_2 \otimes A)C\begin{pmatrix} A^T & 0 \\ 0 & A^T \end{pmatrix}\begin{pmatrix} A & 0 \\ 0 & A \end{pmatrix}\Omega\left(I_2 \otimes A^T\right)$$

$$= (I_2 \otimes A)C\begin{pmatrix} A^TA & 0 \\ 0 & A^TA \end{pmatrix}\Omega\left(I_2 \otimes A^T\right) = (I_2 \otimes A)C\begin{pmatrix} I_t - g_t & 0 \\ 0 & I_t - g_t \end{pmatrix}\Omega\left(I_2 \otimes A^T\right)$$

$$= (I_2 \otimes A)\left\{CI_{2t} - C\begin{pmatrix} g_t & 0 \\ 0 & g_t \end{pmatrix}\right\}\Omega\left(I_2 \otimes A^T\right).$$

Now, from (2.2.6) the product $C\begin{pmatrix} g_t & 0 \\ 0 & g_t \end{pmatrix}$=0. The above expression then becomes:-

$$(I_2 \otimes A)C\Omega\left(I_2 \otimes A^T\right)$$

From the condition of estimability, $(I_2 \otimes A)Z$ is estimable if $(I_2 \otimes A)=(I_2 \otimes A)\Omega C$. Hence the product is

$$(I_2 \otimes A)\left(I_2 \otimes A^T\right) = \begin{pmatrix} AA^T & 0 \\ 0 & AA^T \end{pmatrix} = I_{2(t-1)}.$$

These results apply to any change-over design in which periods and subjects are orthogonal.

If the design is such that each treatment is equally replicated within each period, then

$l_{ik} = b$ = number of appearances of treatment i in period k.

Also $pb = r$ = number of appearances of each treatment in the design

and $b = n/t$.

The C_{ij} matrices then simplify to

$$C_{11} = bpI_t - \left(1/p\right)N_u N_u^T, \qquad C_{12} = M - \left(1/p\right)N_u \tilde{N}_u^T$$

$$C_{22} = b(p-1)I_t - \left[b(p-1)/pt\right]J_{t,t} - \left(1/p\right)\tilde{N}_u \tilde{N}_u^T \tag{2.2.1.4}$$

Further simplification occurs if there is equal replication of treatments within subjects. If this is so then

$$C_{11} = pb(I_t - g_t), \qquad C_{12} = M - b(p-1)g_t$$

$$C_{22} = \left[b\left(p^2 - p - 1\right)/p\right][I_t - g_t] \tag{2.2.1.5}$$

These matrices confirm that the features of the two change-over designs for a 2 by 2 experiment in section 2.1 can be assessed in terms of the matrix M only.

2.3 Factorial Structure and orthogonality

In a factorial experiment, let there be n factors $F_1, F_2, \ldots, F_n$ at levels $m_1, m_2, \ldots, m_n$ respectively. The total number of treatment combinations is $t = \prod_{i=1}^{n} m_i$. Let 1_j be an $m_j \times 1$ vector of ones and A_j be an $\left(m_j - 1\right) \times m_j$ matrix such that $\left(m_j^{-0.5} 1_j, A_j^T\right)$ is orthogonal. Following Mukerjee (1981), let

$$A_j^{x_j} = \begin{cases} m_j^{-0.5} & \text{if } x_j = 0 \\ A_j & \text{if } x_j = 1 \end{cases}$$

For any $x = (x_1, x_2, \ldots, x_n)$ where $x_j = 0,1$ for all j and $x \neq 0$, let

$$A^x = A_1^{x_1} \otimes A_2^{x_2} \otimes \cdots \otimes A_n^{x_n} \tag{2.3.1}$$

The linear functions $A^x \tau$, which are $\prod \left(m_j - 1\right)^{x_j}$ in number, represent a complete set of orthonormal direct treatment contrasts belonging to the generalized interaction $F_1^{x_1} F_2^{x_2} \ldots F_n^{x_n}$.

Similarly, $A^x\rho$ represents residual treatment contrasts. The rows of the $(t-1)\times t$ matrix A in section 2.2 are given by the rows of A^x matrices for all binary numbers x (which are not equal to 0).

Example

Consider a factorial experiment of 2 factors having levels 2 and 3 respectively. The total number of treatments is 6.

$$m_1 = \begin{cases} \left[\frac{1}{\sqrt{2}} \quad \frac{1}{\sqrt{2}}\right] & \text{if } x_1 = 0 \\ \left[-\frac{1}{\sqrt{2}} \quad \frac{1}{\sqrt{2}}\right] & \text{if } x_1 = 1 \end{cases}$$

$$m_2 = \begin{cases} \left[\frac{1}{\sqrt{3}} \quad \frac{1}{\sqrt{3}} \quad \frac{1}{\sqrt{3}}\right] & \text{if } x_2 = 0 \\ \begin{bmatrix} -\frac{1}{\sqrt{2}} & \frac{1}{\sqrt{2}} & 0 \\ \frac{1}{\sqrt{6}} & \frac{1}{\sqrt{6}} & \frac{-2}{\sqrt{6}} \end{bmatrix} & \text{if } x_2 = 1 \end{cases}$$

When $x_1 = 0$ and $x_2 = 1$ then

$$m_1 \otimes m_2 = \left[\frac{1}{\sqrt{2}} \quad \frac{1}{\sqrt{2}}\right] \otimes \begin{bmatrix} -\frac{1}{\sqrt{2}} & \frac{1}{\sqrt{2}} & 0 \\ \frac{1}{\sqrt{6}} & \frac{1}{\sqrt{6}} & \frac{-2}{\sqrt{6}} \end{bmatrix}$$

$$= \begin{bmatrix} -\frac{1}{2} & \frac{1}{2} & 0 & -\frac{1}{2} & \frac{1}{2} & 0 \\ \frac{1}{\sqrt{12}} & \frac{1}{\sqrt{12}} & \frac{-2}{\sqrt{12}} & \frac{1}{\sqrt{12}} & \frac{1}{\sqrt{12}} & \frac{-2}{\sqrt{12}} \end{bmatrix} \qquad (2.3.2)$$

When $x_1 = 1$ and $x_2 = 0$ then

$$m_1 \otimes m_2 = \left[-\frac{1}{\sqrt{2}} \quad \frac{1}{\sqrt{2}}\right] \otimes \left[\frac{1}{\sqrt{3}} \quad \frac{1}{\sqrt{3}} \quad \frac{1}{\sqrt{3}}\right]$$

$$= \left[\frac{-1}{\sqrt{6}} \quad \frac{-1}{\sqrt{6}} \quad \frac{-1}{\sqrt{6}} \quad \frac{1}{\sqrt{6}} \quad \frac{1}{\sqrt{6}} \quad \frac{1}{\sqrt{6}}\right] \qquad (2.3.3)$$

When $x_1 = 1$ and $x_2 = 1$ then

$$m_1 \otimes m_2 = \begin{bmatrix} -\frac{1}{\sqrt{2}} & \frac{1}{\sqrt{2}} \end{bmatrix} \otimes \begin{bmatrix} -\frac{1}{\sqrt{2}} & \frac{1}{\sqrt{2}} & 0 \\ \frac{1}{\sqrt{6}} & \frac{1}{\sqrt{6}} & \frac{-2}{\sqrt{6}} \end{bmatrix}$$

(2.3.4)

$$= \begin{bmatrix} \frac{1}{2} & \frac{-1}{2} & 0 & \frac{-1}{2} & \frac{1}{2} & 0 \\ \frac{-1}{\sqrt{12}} & \frac{-1}{\sqrt{12}} & \frac{2}{\sqrt{12}} & \frac{1}{\sqrt{12}} & \frac{1}{\sqrt{12}} & \frac{-2}{\sqrt{12}} \end{bmatrix}$$

Expressions (2.3.2), (2.3.3) and (2.3.4) give 5 orthonormal contrasts corresponding to the main effects and interactions of the experiment.

For factorial experiments with a control treatment, the total number of treatments is equal to $t^* = (\prod_{i=1}^{n} m_i) + 1 = t + 1$. The control treatment will be the "zeroth treatment " listed first. An additional contrast of the control versus the rest of the treatments is considered. This contrast is given by $a^T = \begin{bmatrix} \frac{t^* - 1}{\sqrt{t^*(t^* - 1)}} & \frac{-1}{\sqrt{t^*(t^* - 1)}} & \frac{-1}{\sqrt{t^*(t^* - 1)}} & \cdots & \frac{-1}{\sqrt{t^*(t^* - 1)}} \end{bmatrix}$

Suppose A is the $(t-1) \times t$ matrix of orthonormal contrasts for a factorial experiment and is obtained as indicated in equation (2.3.1). The $(t^* - 1) \times t^*$ matrix of orthonormal contrasts for such an experiment with a control treatment is given by $A^* = \begin{bmatrix} a^T \\ 0_{t^* - 1}, A \end{bmatrix}$ where $0_{t^* - 1}$ is a $(t^* - 1)$ vector of zeroes.

Also $A^* A^{*T} = I_{t^* - 1}$ and $A^{*T} A^* = I_{t^*} - g_{t^*}$ (2.3.5)

Factorial change-over designs are classified according to the degree of orthogonality. It is desirable to have direct treatment effects orthogonal to both period and subject effects. This occurs when there is equal replication of treatments within both periods and subjects. When this is the case, the matrix M plays a key role in the design as seen in equation (2.2.1.5). It

is also desirable to have direct and residual effects orthogonal to each other. Partitioning matrix Ω into $t \times t$ matrices denoted Ω_{ij} (i,j =1,2), the requirement of orthogonality between direct treatment effects and residual treatment effects is $A\Omega_{12}A^T = 0$ from equation (2.2.1.2). That is there is no correlation between the estimates of direct treatment effects and estimates of residual treatment effects. This would imply that $\left(V^{-1}\right)_{ij} = 0$ for $i = 1,...,t$; $j = t+1,...,2t$.

From (2.2.1.3) this becomes

$$AC_{12}A^T = 0$$

This implies that

$$A^T AC_{12}A^T A = 0$$

Since $A^T A = I_t - g_t$, the left hand side becomes

$$[I_t - g_t]C_{12}[I_t - g_t] = [C_{12} - C_{12}g_t][I_t - g_t] = 0$$

From (2.2.6) $C_{12}g_t = 0$, therefore $C_{12} = 0$.

In other words, a change-over design has direct and residual effects orthogonal only if $C_{12} = 0$. This occurs when each treatment follows itself and every other treatment an equal number of times, say "e " times. The matrix M for such a design would be $M = eJ_{t,t}$.

Berenblut(1964) has made available designs that have this property for t treatments, $2t$ periods and t^2 subjects. An example for such a design is as follows:

Example:

$t = 3$

	Subject								
Period	1	2	3	4	5	6	7	8	9
1	0	1	2	0	1	2	0	1	2
2	2	0	1	1	2	0	0	1	2
3	1	2	0	1	2	0	1	2	0
4	1	2	0	0	1	2	2	0	1
5	2	0	1	2	0	1	2	0	1
6	0	1	2	2	0	1	1	2	0

Cheng and Wu (1980) have also made available designs with this property for t treatments, p periods and n experimental units, where t^2 is a divisor of n and p/t is an even integer. The factorial structure in factorial change-over designs has less stringent conditions. A design is said to have **factorial structure** if and only if

1. estimates of direct treatment contrasts belonging to different factorial effects are orthogonal.
2. estimates of residual treatment effects belonging to different factorial effects are orthogonal
3. estimates of direct treatment contrasts and estimates of residual treatment contrasts belonging to different factorial effects are orthogonal. (2.3.6)

In terms of the covariance matrix V in (2.2.1.2), these conditions are that $A^x(\Omega)_{ij}(A^y)^T = 0$ for all $x \neq y$, $i,j=1,2$ where A^x is as given in (2.3.1).

Since V under suitable permutations of rows and columns can be expressed as a block diagonal matrix, and so can V^{-1} under similar permutations, it therefore follows that a factorial change-over design has a factorial structure if and only if

$A^x C_{ij} A^{y^T} = 0$ for all $x \neq y$, $i,j=1,2$ (2.3.7)

For design 1 in section 2.1, the matrix C and matrix A are as follows

$$C=\begin{bmatrix} 6 & -2 & -2 & -2 & -1.5 & .5 & .5 & .5 \\ -2 & 6 & -2 & -2 & .5 & -1.5 & .5 & .5 \\ -2 & -2 & 6 & -2 & .5 & .5 & -1.5 & .5 \\ -2 & -2 & -2 & 6 & .5 & .5 & .5 & -1.5 \\ -1.5 & .5 & .5 & .5 & 4.125 & -1.375 & -1.375 & -1.375 \\ .5 & -1.5 & .5 & .5 & -1.375 & 4.125 & -1.375 & -1.375 \\ .5 & .5 & -1.5 & .5 & -1.375 & -1.375 & 4.125 & -1.375 \\ .5 & .5 & .5 & -1.5 & -1.375 & -1.375 & -1.375 & 4.125 \end{bmatrix}$$

$$A=\begin{bmatrix} -.5 & .5 & -.5 & .5 \\ -.5 & -.5 & .5 & .5 \\ .5 & -.5 & -.5 & .5 \end{bmatrix}$$

It can be seen that design 1 satisfies equation (2.3.7) hence it has factorial structure. It can also be verified that design 2 also has factorial structure.

Mukerjee (1979) has established necessary and sufficient conditions for a block design with equal replication and constant block size to have factorial structure. These conditions also apply to change-over designs.

Structure K

This definition is given by Mukerjee. A $t \times t$ matrix $\boldsymbol{C}$ (where $t = \prod m_i$, i = 1,2...n) is said to have **structure *K*** if it can be expressed as a linear combination of Kroenecker products of proper matrices $m_1, m_2 \cdots m_n$ (see section 1.5). That is

$$C = \sum_{g=1}^{w} \xi_g (V_{g1} \otimes V_{g2} \otimes \cdots \otimes V_{gn})$$

where w is a positive integer, $\xi_1, \xi_2, \cdots, \xi_w$ are some real numbers and for each g, V_{gj} is a proper matrix of order m_j (j = 1, 2...., n). It has been shown by Mukerjee that $A^x C (A^y)^T = 0$ for $x \neq y$, if and only if $\boldsymbol{C}$ has structure $\boldsymbol{K}$. It follows then that a factorial change-over design has a factorial structure if and only if C_{ij} ($i,j=1,2$) in (2.2.4) have structure $\boldsymbol{K}$.

The matrix M for a design in which there is equal replication of treatments both within subjects and periods is a proper matrix. It therefore follows from (2.2.1.5) that designs having equal replication both within subjects and periods have the factorial structure. Designs in which the number of periods p is less than t but have equal replication of treatments within periods have their C_{ij} matrices given in (2.2.1.4). In this case, C_{11}, C_{12} and C_{22}

have structure $\boldsymbol{K}$ if and only if $N_u N_u^T$, $N_u \tilde{N}_u^T$ and $\tilde{N}_u \tilde{N}_u^T$ respectively have structure $\boldsymbol{K}$. Balanced incomplete block (BIB) designs have structure $\boldsymbol{K}$. A number of partially balanced incomplete blocks of 2 associate classes (PBIB/2) having structure $\boldsymbol{K}$ have been made available. Change-over designs derived from BIB designs and these PBIB/2 designs also have structure $\boldsymbol{K}$.

2.4 Variances for direct and residual treatment contrasts

The coefficient matrices for estimating the direct and residual effects of the treatments separately are respectively

$C_d = C_{11} - C_{12}C_{22}^{-}C_{21}$

$C_r = C_{22} - C_{21}C_{11}^{-}C_{12}$

where C_{ij} $\boldsymbol{i,j=1,2}$ are as given in (2.2.4).

The variance for a given contrast of treatments, either direct or residual, takes the form of equation (1.5.2). Following is a table giving the variances (divided by σ^2) for different effects for design 1 and design 2.

Table 2.3: Variances (divided by σ^2) for designs 1 and 2

		Design 1	Design 2
Main effects of A	direct	0.138	0.125
	residual	0.200	0.182
Main effects of B	direct	0.138	0.125
	residual	0.200	0.182
Interaction AB	direct	0.138	0.688
	residual	0.200	1.000

It is evident that "balancing" for the different levels of both factor A and factor B does reduce the variances for the main effects. Factorial change-over designs should be constructed having in mind which of the effects are most important.

The theory concerning the factorial structure for factorial change-over designs is directly applicable to change-over designs for factorial experiments with a control. This follows from the choice of the matrix A^* (as seen in (2.3.5)) whose rows represent the orthonormal contrasts of a factorial experiment with a control. A change-over design for a factorial experiment with a control will have the factorial structure if it satisfies conditions (2.3.6) as well as the following conditions; the estimate of the contrast corresponding to control versus treatments will be orthogonal to;

1. estimates of direct treatment contrasts belonging to each factorial effect.
2. estimates of residual treatment contrasts belonging to each factorial effect.

Chapter 3
Designs for Factorial Experiments with a Control

Change-over designs for factorial experiments with a control are considered. Designs for which each given level of a given factor follows itself and every other level of the factor an equal or nearly equal number of times are compared with designs derived from Williams squares. Strongly balanced uniform designs are considered for the case of factorial experiments with a control. A method of construction for a change-over design for a $2 \times k$ ($k = 2,3,4...$) experiment + control is provided.

3.1 Introduction

Consider a factorial experiment in which there are two factors, say A and B, of levels m and n respectively. There are a total of mn treatment combinations. An additional treatment, say treatment C, is included in the experiment and taken as the control treatment. If the conditions of the experiment are such that residual effects are to be considered, then the experiment would require a change-over design for a factorial experiment with a control.

Example

In a dairy products firm, milk prepared from reconstituted whole milk powder plus different additives is compared. The collection of different additives of the milk is as follows:

control (base powder)

base + cheese permeate @ 5%

base + cheese permeate @ 10%

base + rennet permeate @ 5%

base + rennet permeate @ 10%

The factorial nature of this experiment involves the type of permeate and the addition rate. The control treatment is the reconstituted whole milk powder. Tasters are supposed to give their impression of the 5 different kinds of milk. A change-over design is the most appropriate because of the carry-over effects expected with each taste excluding the first that is made by each taster.

The designs considered here have the factorial structure described in chapter 2. Designs that are efficient at estimating the main effects of a given factor are pursued by considering how different effects of the factor follow each other. This follows Fletcher and John's (1985) postulation which says that a design in which each level of a given factor follows itself and every other level of the given factor an equal number of times is better in the estimation of the main effect of the given factor than a design in which the levels of the given factor do not follow each other an equal number of times.

3.2: 2 by 2 experiment with a control

The 5 treatments of this experiment are labelled as follows:-
0 as the control treatment, 1 as a_1b_1, 2 as a_2b_1, 3 as a_1b_2, 4 as a_2b_2. Three designs are listed below and the relationship of the different levels of the factors are also listed. The notation is the same as in chapter 2.

Design 3.2.1

This design is derived from two 5 by 5 Williams squares (see Table 1.2). Each treatment follows every other treatment twice. The design has the following relationships:

$a_1 \rightarrow a_1 = 4$ times	$b_1 \rightarrow b_1 = 4$ times	$a_1 \rightarrow b_1 = 6$ times	$b_1 \rightarrow a_1 = 6$ times
$a_1 \rightarrow a_2 = 8$ times	$b_1 \rightarrow b_2 = 8$ times	$a_1 \rightarrow b_2 = 6$ times	$b_1 \rightarrow a_2 = 6$ times
$a_2 \rightarrow a_1 = 8$ times	$b_2 \rightarrow b_1 = 8$ times	$a_2 \rightarrow b_1 = 6$ times	$b_2 \rightarrow a_1 = 6$ times
$a_2 \rightarrow a_2 = 4$ times	$b_2 \rightarrow b_2 = 4$ times	$a_2 \rightarrow b_2 = 6$ times	$b_2 \rightarrow a_2 = 6$ times

Design 3.2.2

	Experimental unit									
Period	1	2	3	4	5	6	7	8	9	10
1	0	1	2	3	4	0	1	2	3	4
2	2	0	4	1	3	3	0	1	4	2
3	4	3	0	2	1	4	2	3	0	1
4	3	4	1	0	2	2	4	0	1	3
5	1	2	3	4	0	1	3	4	2	0

This design has the following relationships:

$a_1 \rightarrow a_1 = 6$ times	$b_1 \rightarrow b_1 = 6$ times	$a_1 \rightarrow b_1 = 6$ times	$b_1 \rightarrow a_1 = 6$ times
$a_1 \rightarrow a_2 = 6$ times	$b_1 \rightarrow b_2 = 6$ times	$a_1 \rightarrow b_2 = 6$ times	$b_1 \rightarrow a_2 = 6$ times
$a_2 \rightarrow a_1 = 6$ times	$b_2 \rightarrow b_1 = 6$ times	$a_2 \rightarrow b_1 = 6$ times	$b_2 \rightarrow a_1 = 6$ times
$a_2 \rightarrow a_2 = 6$ times	$b_2 \rightarrow b_2 = 6$ times	$a_2 \rightarrow b_2 = 6$ times	$b_2 \rightarrow a_2 = 6$ times

Design 3.2.3

	Experimental unit									
Period	1	2	3	4	5	6	7	8	9	10
1	0	1	2	3	4	1	2	3	4	0
2	2	3	4	0	1	3	4	0	1	2
3	4	0	1	2	3	4	0	1	2	3
4	3	4	0	1	2	2	3	4	0	1
5	1	2	3	4	0	0	1	2	3	4

The design has the following relationships:-

$a_1 \rightarrow a_1 = 6$ times	$b_1 \rightarrow b_1 = 2$ times	$a_1 \rightarrow b_1 = 5$ times	$b_1 \rightarrow a_1 = 5$ times
$a_1 \rightarrow a_2 = 6$ times	$b_1 \rightarrow b_2 = 10$ times	$a_1 \rightarrow b_2 = 7$ times	$b_1 \rightarrow a_2 = 7$ times
$a_2 \rightarrow a_1 = 6$ times	$b_2 \rightarrow b_1 = 10$ times	$a_2 \rightarrow b_1 = 7$ times	$b_2 \rightarrow a_1 = 7$ times
$a_2 \rightarrow a_2 = 6$ times	$b_2 \rightarrow b_2 = 2$ times	$a_2 \rightarrow b_2 = 5$ times	$b_2 \rightarrow a_2 = 5$ times

The variances of the contrasts of direct effects as well as the residual effects are calculated using equation (1.5.2) in the first chapter. The contrasts of the various effects are given by the rows of the matrix A^* of section 2.3. The variances (divided by σ^2) of the 3 designs are given in the following table:

Table 3.2.1: Variances (divided by σ^2) for designs 3.2.1, 3.2.2 and 3.2.3

		Design 3.2.1	Design 3.2.2	Design 3.2.3
Control	direct effect	0.106	0.106	0.115
	residual effect	0.139	0.139	0.152
Main effect of A	direct effect	0.106	0.100	0.102
	residual effect	0.139	0.132	0.134
Main effect of B	direct effect	0.106	0.100	0.129
	residual effect	0.139	0.132	0.170
Interaction AB	direct effect	0.106	0.190	0.115
	residual effect	0.139	0.250	0.152

Design 3.2.2 has each level of A following itself and every other level of A an equal number of times. Levels of B have a similar relationship. Also in the same design, each level of A follows each level of B an equal number of times. Levels of B follow levels of A in a similar way. The control treatment follows every treatment combination an equal number of times. This design has the least variances in the estimation of main effects of A and also the main

effects of B. Design 3.2.3 which has each level of A following itself and every other level of A an equal number of times has less variance for the estimation of the main effects of A than design 3.2.1. On the other hand, design 3.2.1 does best in the estimation of interaction effects. This is due to the fact that each treatment follows every other treatment an equal number of times in design 3.2.1. Design 3.2.2 offers the poorest estimates in the estimation of interaction effects despite the fact that it has levels of A following levels of B an equal number of times.

From the above three designs, it looks reasonable to pursue designs that are efficient in the estimation of main effects of a given factor by looking at how the levels of the given factor follow each other. The estimation of interaction effects is best done by a design which has each treatment combination following every other treatment combination an equal or nearly equal number of times. Design 3.2.2 was constructed by a search on the complete enumeration of combinations of all possible latin squares of side 5. It would require a lot of computer time to search for designs for which $t = 2k + 1 > 5$ ($k = 2,3,4...$) with the kind of properties that design 3.2.2 has, if they exist at all.

In the next section, several other designs will be looked at. The designs considered are for $2 \times k$ ($k = 2,3,4...$) experiment + control. The variances of these designs will be investigated with the background knowledge of how the different levels of the designs follow each other. The treatments are coded as follows:

Treatment combination	Coding
Control	0
a_1b_1	1
a_2b_1	2
⋮	⋮
a_1b_k	$2k-1$
a_2b_k	$2k$

3.3: Designs for 2 by k experiment + control (k=3,4,5)

3.3.1 2 by 3 experiment with a control

Design 3.3.1.1

This design is derived from Williams squares. The design has the following relationships:-

$a_1 \rightarrow a_1=12$	$b_1 \rightarrow b_2=8$	$a_1 \rightarrow b_1=10$	$a_2 \rightarrow b_1=10$
$a_1 \rightarrow a_2=18$	$b_2 \rightarrow b_1=8$	$b_1 \rightarrow a_1=10$	$b_1 \rightarrow a_2=10$
$a_2 \rightarrow a_1=18$	$b_1 \rightarrow b_3=8$	$a_1 \rightarrow b_2=10$	$a_2 \rightarrow b_2=10$
$a_2 \rightarrow a_2=12$	$b_3 \rightarrow b_1=8$	$b_2 \rightarrow a_1=10$	$b_2 \rightarrow a_2=10$
$b_1 \rightarrow b_1=4$	$b_2 \rightarrow b_3=8$	$a_1 \rightarrow b_3=10$	$a_2 \rightarrow b_3=10$
$b_2 \rightarrow b_2=4$	$b_3 \rightarrow b_2=8$	$b_3 \rightarrow a_1=10$	$b_3 \rightarrow a_2=10$
$b_3 \rightarrow b_3=4$			

Design 3.3.1.2

This design is formed by two 7×7 latin squares whose initial columns are 0,4,5,3,6,1,2, and 4,2,1,6,3,5,0. The other columns are generated from the initial columns by cyclic development (mod 7). The relationships of the design are as follows:

$a_1 \rightarrow a_1$=15	$b_1 \rightarrow b_2$=9	$a_1 \rightarrow b_1$=9	$a_2 \rightarrow b_1$=11
$a_1 \rightarrow a_2$=15	$b_2 \rightarrow b_1$=9	$b_1 \rightarrow a_1$=9	$b_1 \rightarrow a_2$=10
$a_2 \rightarrow a_1$=15	$b_1 \rightarrow b_3$=7	$a_1 \rightarrow b_2$=11	$a_2 \rightarrow b_2$=10
$a_2 \rightarrow a_2$=15	$b_3 \rightarrow b_1$=8	$b_2 \rightarrow a_1$=10	$b_2 \rightarrow a_2$=11
$b_1 \rightarrow b_1$=3	$b_2 \rightarrow b_3$=9	$a_1 \rightarrow b_3$=10	$a_2 \rightarrow b_3$=9
$b_2 \rightarrow b_2$=3	$b_3 \rightarrow b_2$=9	$b_3 \rightarrow a_1$=11	$b_3 \rightarrow a_2$=9
$b_3 \rightarrow b_3$=3			

Design 3.3.1.3

This design is formed by two 7×7 latin squares whose initial columns are 0,4,6,1,3,2,5 and 5,2,3,1,6,4,0. The other columns are generated from the initial columns by cyclic development (mod 7). The relationships of the design are as follows:-

$a_1 \rightarrow a_1$=16	$b_1 \rightarrow b_2$=9	$a_1 \rightarrow b_1$=9	$a_2 \rightarrow b_1$=11
$a_1 \rightarrow a_2$=14	$b_2 \rightarrow b_1$=9	$b_1 \rightarrow a_1$=9	$b_1 \rightarrow a_2$=11
$a_2 \rightarrow a_1$=14	$b_1 \rightarrow b_3$=9	$a_1 \rightarrow b_2$=10	$a_2 \rightarrow b_2$=10
$a_2 \rightarrow a_2$=16	$b_3 \rightarrow b_1$=9	$b_2 \rightarrow a_1$=10	$b_2 \rightarrow a_2$=10
$b_1 \rightarrow b_1$=2	$b_2 \rightarrow b_3$=9	$a_1 \rightarrow b_3$=11	$a_2 \rightarrow b_3$=9
$b_2 \rightarrow b_2$=2	$b_3 \rightarrow b_2$=9	$b_3 \rightarrow a_1$=11	$b_3 \rightarrow a_2$=9
$b_3 \rightarrow b_3$=2			

The variances (divided by σ^2) of these designs are given below. There are 2 degrees of freedom associated with the main effects of factor *B* and also with the interaction effects *AB*. The variances for these effects are the averages of the variances of the contrasts associated with each of them.

Table 3.3.1.1: Variances (divided by σ^2) for designs 3.3.1.1, 3.3.1.2 and 3.3.1.3

		Design 3.3.1.1	Design 3.3.1.2	Design 3.3.1.3
Control	direct effect	0.07321	0.07566	0.07550
	residual effect	0.08750	0.09042	0.09024
Main effect of A	direct effect	0.07321	0.07240	0.07234
	residual effect	0.08750	0.08653	0.08645
Main effect of B	direct effect	0.07321	0.07609	0.07742
	residual effect	0.08750	0.09093	0.09253
Interaction AB	direct effect	0.07321	0.07687	0.07517
	residual effect	0.08750	0.09187	0.08984

3.3.2 2 by 4 experiment with a control

These designs have 9 treatments, 9 periods and 18 experimental units.

Design 3.3.2.1

This design is derived from Williams squares. The relationships of the design are as shown below:-

$a_1 \rightarrow a_1=24$	$b_2 \rightarrow b_1=8$	$b_3 \rightarrow b_4=8$	$b_4 \rightarrow a_1=14$
$a_1 \rightarrow a_2=32$	$b_1 \rightarrow b_3=8$	$b_4 \rightarrow b_3=8$	$a_2 \rightarrow b_1=14$
$a_2 \rightarrow a_1=32$	$b_3 \rightarrow b_1=8$	$a_1 \rightarrow b_1=14$	$b_1 \rightarrow a_2=14$
$a_2 \rightarrow a_2=24$	$b_1 \rightarrow b_4=8$	$b_1 \rightarrow a_1=14$	$a_2 \rightarrow b_2=14$
$b_1 \rightarrow b_1=4$	$b_4 \rightarrow b_1=8$	$a_1 \rightarrow b_2=14$	$b_2 \rightarrow a_2=14$
$b_2 \rightarrow b_2=4$	$b_2 \rightarrow b_3=8$	$b_2 \rightarrow a_1=14$	$a_2 \rightarrow b_3=14$
$b_3 \rightarrow b_3=4$	$b_3 \rightarrow b_2=8$	$a_1 \rightarrow b_3=14$	$b_3 \rightarrow a_2=14$
$b_4 \rightarrow b_4=4$	$b_2 \rightarrow b_4=8$	$b_3 \rightarrow a_1=14$	$a_2 \rightarrow b_4=14$
$b_1 \rightarrow b_2=8$	$b_4 \rightarrow b_2=8$	$a_1 \rightarrow b_4=14$	$b_4 \rightarrow a_2=14$

Design 3.3.2.2

This design is made by two 9×9 latin squares whose initial columns are 0,1,3,5,2,6,4,8,7 and 7,8,4,6,2,5,3,1,0. The rest of the columns are generated from the initial columns by cyclic development (mod 9). The relationships of this design are given below. Each level of factor A follows itself and the other level of A an equal number of times.

$a_1 \rightarrow a_1$=28	$b_2 \rightarrow b_1$=9	$b_3 \rightarrow b_4$=9	$b_4 \rightarrow a_1$=14
$a_1 \rightarrow a_2$=28	$b_1 \rightarrow b_3$=7	$b_4 \rightarrow b_3$=9	$a_2 \rightarrow b_1$=14
$a_2 \rightarrow a_1$=28	$b_3 \rightarrow b_1$=7	$a_1 \rightarrow b_1$=13	$b_1 \rightarrow a_2$=14
$a_2 \rightarrow a_2$=28	$b_1 \rightarrow b_4$=7	$b_1 \rightarrow a_1$=13	$a_2 \rightarrow b_2$=15
$b_1 \rightarrow b_1$=4	$b_4 \rightarrow b_1$=7	$a_1 \rightarrow b_2$=14	$b_2 \rightarrow a_2$=15
$b_2 \rightarrow b_2$=4	$b_2 \rightarrow b_3$=9	$b_2 \rightarrow a_1$=14	$a_2 \rightarrow b_3$=14
$b_3 \rightarrow b_3$=4	$b_3 \rightarrow b_2$=9	$a_1 \rightarrow b_3$=15	$b_3 \rightarrow a_2$=14
$b_4 \rightarrow b_4$=4	$b_2 \rightarrow b_4$=7	$b_3 \rightarrow a_1$=15	$a_2 \rightarrow b_4$=13
$b_1 \rightarrow b_2$=9	$b_4 \rightarrow b_2$=7	$a_1 \rightarrow b_4$=14	$b_4 \rightarrow a_2$=13

Design 3.3.2.3

This design is made by two 9×9 latin squares whose initial columns are 0,7,1,3,8,5,4,6,2 and 2,6,4,5,8,3,1,7,0. The other columns are generated from the initial columns by cyclic development (mod 9). The relationships of the design are given below:

$a_1 \rightarrow a_1$=30	$b_2 \rightarrow b_1$=9	$b_3 \rightarrow b_4$=9	$b_4 \rightarrow a_1$=15
$a_1 \rightarrow a_2$=26	$b_1 \rightarrow b_3$=8	$b_4 \rightarrow b_3$=9	$a_2 \rightarrow b_1$=15
$a_2 \rightarrow a_1$=26	$b_3 \rightarrow b_1$=8	$a_1 \rightarrow b_1$=13	$b_1 \rightarrow a_2$=15
$a_2 \rightarrow a_2$=30	$b_1 \rightarrow b_4$=9	$b_1 \rightarrow a_1$=13	$a_2 \rightarrow b_2$=14
$b_1 \rightarrow b_1$=2	$b_4 \rightarrow b_1$=9	$a_1 \rightarrow b_2$=14	$b_2 \rightarrow a_2$=14
$b_2 \rightarrow b_2$=2	$b_2 \rightarrow b_3$=9	$b_2 \rightarrow a_1$=14	$a_2 \rightarrow b_3$=14
$b_3 \rightarrow b_3$=2	$b_3 \rightarrow b_2$=9	$a_1 \rightarrow b_3$=14	$b_3 \rightarrow a_2$=14
$b_4 \rightarrow b_4$=2	$b_2 \rightarrow b_4$=8	$b_3 \rightarrow a_1$=14	$a_2 \rightarrow b_4$=13
$b_1 \rightarrow b_2$=9	$b_4 \rightarrow b_2$=8	$a_1 \rightarrow b_4$=15	$b_4 \rightarrow a_2$=13

The variances (divided by σ^2) of these designs are given below. There are 3 degrees of freedom associated with the main effects of factor *B* and also with the interaction effects *AB*. The variances for these effects are the averages of the variances of the contrasts associated with each of them.

Table 3.3.2.1: Variances (divided by σ^2) for designs 3.3.2.1, 3.3.2.2 and 3.3.2.3

		Design 3.3.2.1	Design 3.3.2.2	Design 3.3.2.3
Control	direct effect	0.05635	0.05735	0.05730
	residual effect	0.06429	0.06543	0.06537
Main effect of A	direct effect	0.05635	0.05608	0.05605
	residual effect	0.06428	0.06398	0.06395
Main effect of B	direct effect	0.05635	0.05691	0.05799
	residual effect	0.06429	0.06492	0.06616
Interaction AB	direct effect	0.05635	0.05821	0.05702
	residual effect	0.06429	0.06641	0.06506

3.3.3 2 by 5 experiment with a control

The designs for this experiment have 11 treatments, 11 periods and 22 experimental units.

Design 3.3.3.1

This design is derived from Williams squares. The relationships of this design are as shown below:

$a_1 \rightarrow a_1=40$
$a_1 \rightarrow a_2=50$
$a_2 \rightarrow a_1=50$
$a_2 \rightarrow a_2=40$
$b_1 \rightarrow b_1=4$
$b_2 \rightarrow b_2=4$
$b_3 \rightarrow b_3=4$
$b_4 \rightarrow b_4=4$
$b_5 \rightarrow b_5=4$
$b_1 \rightarrow b_2=8$
$b_2 \rightarrow b_1=8$
$b_1 \rightarrow b_3=8$
$b_3 \rightarrow b_1=8$
$b_1 \rightarrow b_4=8$
$b_4 \rightarrow b_1=8$
$b_1 \rightarrow b_5=8$
$b_5 \rightarrow b_1=8$
$b_2 \rightarrow b_3=8$
$b_3 \rightarrow b_2=8$
$b_2 \rightarrow b_4=8$
$b_4 \rightarrow b_2=8$
$b_2 \rightarrow b_5=8$
$b_5 \rightarrow b_2=8$
$b_3 \rightarrow b_4=8$
$b_4 \rightarrow b_3=8$
$b_3 \rightarrow b_5=8$
$b_5 \rightarrow b_3=8$
$b_4 \rightarrow b_5=8$
$b_5 \rightarrow b_4=8$
$a_1 \rightarrow b_1=18$
$b_1 \rightarrow a_1=18$
$a_1 \rightarrow b_2=18$
$b_2 \rightarrow a_1=18$
$a_1 \rightarrow b_3=18$
$b_3 \rightarrow a_1=18$
$a_1 \rightarrow b_4=18$
$b_4 \rightarrow a_1=18$
$a_1 \rightarrow b_5=18$
$b_5 \rightarrow a_1=18$
$a_2 \rightarrow b_1=18$
$b_1 \rightarrow a_2=18$
$a_2 \rightarrow b_2=18$
$b_2 \rightarrow a_2=18$
$a_2 \rightarrow b_3=18$
$b_3 \rightarrow a_2=18$
$a_2 \rightarrow b_4=18$
$b_4 \rightarrow a_2=18$
$a_2 \rightarrow b_5=18$
$b_5 \rightarrow a_2=18$

Design 3.3.3.2

This design is made up of two 11×11 latin squares whose initial columns are 0,4,1,3,5,10,2,9,8,6,7 and 0,3,5,7,1,9,8,6,4,10,2. The rest of the columns are generated by cyclic development of the initial columns (mod 11). The relationships of this design are as follows:-

$a_1 \rightarrow a_1$=45
$a_1 \rightarrow a_2$=45
$a_2 \rightarrow a_1$=45
$a_2 \rightarrow a_2$=45
$b_1 \rightarrow b_1$=3
$b_2 \rightarrow b_2$=3
$b_3 \rightarrow b_3$=3
$b_4 \rightarrow b_4$=3
$b_5 \rightarrow b_5$=3
$b_1 \rightarrow b_2$=10
$b_2 \rightarrow b_1$=12
$b_1 \rightarrow b_3$=5
$b_3 \rightarrow b_1$=7
$b_1 \rightarrow b_4$=6
$b_4 \rightarrow b_1$=5
$b_1 \rightarrow b_5$=11
$b_5 \rightarrow b_1$=8
$b_2 \rightarrow b_3$=10
$b_3 \rightarrow b_2$=12
$b_2 \rightarrow b_4$=5
$b_4 \rightarrow b_2$=7
$b_2 \rightarrow b_5$=6
$b_5 \rightarrow b_2$=5
$b_3 \rightarrow b_4$=10
$b_4 \rightarrow b_3$=12
$b_3 \rightarrow b_5$=5
$b_5 \rightarrow b_3$=7
$b_4 \rightarrow b_5$=10
$b_5 \rightarrow b_4$=12
$a_1 \rightarrow b_1$=17
$b_1 \rightarrow a_1$=16
$a_1 \rightarrow b_2$=19
$b_2 \rightarrow a_1$=19
$a_1 \rightarrow b_3$=18
$b_3 \rightarrow a_1$=19
$a_1 \rightarrow b_4$=17
$b_4 \rightarrow a_1$=18
$a_1 \rightarrow b_5$=19
$b_5 \rightarrow a_1$=18
$a_2 \rightarrow b_1$=18
$b_1 \rightarrow a_2$=19
$a_2 \rightarrow b_2$=18
$b_2 \rightarrow a_2$=17
$a_2 \rightarrow b_3$=19
$b_3 \rightarrow a_2$=18
$a_2 \rightarrow b_4$=19
$b_4 \rightarrow a_2$=29
$a_2 \rightarrow b_5$=16
$b_5 \rightarrow a_2$=17

Design 3.3.3.3

This design is made up of two 11×11 latin squares whose initial squares are 0,6,7,5,8,4,9,2,10,1,3 and 0,9,7,10,6,1,5,2,4,3,8. The rest of the columns are generated by cyclic development (mod 11) of the initial columns. The relationship of the design are as given below:

$a_1 \rightarrow a_1$=48
$a_1 \rightarrow a_2$=42
$a_2 \rightarrow a_1$=42
$a_2 \rightarrow a_2$=48
$b_1 \rightarrow b_1$=2
$b_2 \rightarrow b_2$=2
$b_3 \rightarrow b_3$=2
$b_4 \rightarrow b_4$=2
$b_5 \rightarrow b_5$=2
$b_1 \rightarrow b_2$=9
$b_2 \rightarrow b_1$=9
$b_1 \rightarrow b_3$=8
$b_3 \rightarrow b_1$=8
$b_1 \rightarrow b_4$=8
$b_4 \rightarrow b_1$=8
$b_1 \rightarrow b_5$=9
$b_5 \rightarrow b_1$=9
$b_2 \rightarrow b_3$=9
$b_3 \rightarrow b_2$=9
$b_2 \rightarrow b_4$=8
$b_4 \rightarrow b_2$=8
$b_2 \rightarrow b_5$=8
$b_5 \rightarrow b_2$=8
$b_3 \rightarrow b_4$=9
$b_4 \rightarrow b_3$=9
$b_3 \rightarrow b_5$=8
$b_5 \rightarrow b_3$=8
$b_4 \rightarrow b_5$=9
$b_5 \rightarrow b_4$=9
$a_1 \rightarrow b_1$=17
$b_1 \rightarrow a_1$=17
$a_1 \rightarrow b_2$=18
$b_2 \rightarrow a_1$=18
$a_1 \rightarrow b_3$=18
$b_3 \rightarrow a_1$=18
$a_1 \rightarrow b_4$=18
$b_4 \rightarrow a_1$=18
$a_1 \rightarrow b_5$=19
$b_5 \rightarrow a_1$=19
$a_2 \rightarrow b_1$=19
$b_1 \rightarrow a_2$=19
$a_2 \rightarrow b_2$=18
$b_2 \rightarrow a_2$=18
$a_2 \rightarrow b_3$=18
$b_3 \rightarrow a_2$=18
$a_2 \rightarrow b_4$=18
$b_4 \rightarrow a_2$=18
$a_2 \rightarrow b_5$=17
$b_5 \rightarrow a_2$=17

The variances (divided by σ^2) of these designs are as given below. There are 4 degrees of freedom associated with the main effects of factor *B* and also with the interaction effects *AB*. The variances for these effects are the averages of the variances of the contrasts associated with each of them.

Table 3.3.3.1: Variances (divided by σ^2) for designs 3.3.3.1, 3.3.3.2 and 3.3.3.3

		Design 3.3.3.1	Design 3.3.3.2	Design 3.3.3.3
Control	direct effect	0.04588	0.04713	0.04636
	residual effect	0.05093	0.05232	0.05146
Main effect of A	direct effect	0.04588	0.04569	0.04575
	residual effect	0.05093	0.05072	0.05079
Main effect of B	direct effect	0.04588	0.04756	0.04668
	residual effect	0.05093	0.05280	0.05182
Interaction AB	direct effect	0.04588	0.04707	0.04618
	residual effect	0.05093	0.05225	0.05127

3.4 Concluding remarks on these designs

The differences in the variances of the various effects in the designs given in the previous section range from small to minuscule. There is an indication that designs in which each level of a given factor follows itself and every other level of the given factor an equal number of times tend to have low variances for the estimation of the main effects of that particular factor. The results also show that these designs do not necessarily give the best estimates of the main effects of that particular factor. Design 3.3.1.3 does better than design 3.3.1.2 in the estimation of the main effects of factor *A* (see table 3.3.1.1). So does design 3.3.2.3 when compared to design 3.3.2.2 (see table 3.3.2.1). The results in the previous section also make it apparent that the efficiency in the estimation of the interaction effects depends on how nearly equal the treatment combinations follow each other. The efficiency in the estimation of interaction effects does not depend on how the levels of the different factors follow each other as illustrated by most of the designs. The designs in which each treatment combination follows every other treatment combination an equal or nearly equal number of times tend to do better in the estimation of interaction effects.

The results in the previous section also show that all the alternative designs provided make an improvement (however small) on the Williams squares designs in the estimation of the main effects of A at the expense of precision in the estimation of all other effects. The largest improvement is on the 2×2 factorial experiment with a control treatment. The improvement diminishes as k becomes larger. The gains made by these designs over the Williams square designs are small and provide little motivation for their use over Williams squares designs in factorial experiments with a control treatment.

Designs 3.3.1.3, 3.3.2.3 and 3.3.3.3 perform better in 3 out of 4 of the effects of treatment contrasts estimated than the designs which have each level of factor A following itself and all other levels of factor A equally often. The average variances of the $(t - 1)$ orthogonal treatment contrasts for the designs 3.3.1.3, 3.3.2.3 and 3.3.3.3 are lower than the average variances for the designs balanced for factor A. The table below gives the average variances for these designs and for the designs balanced for the levels of factor A. The designs balanced for factor A are designs 3.3.1.2, 3.3.2.2 and 3.3.3.2.

Table 3.4.1: Average variances for $(t - 1)$ treatment contrasts

	Design 3.3.1.2	Design 3.3.1.3
Direct effects	0.07566	0.07550
Residual effects	0.09043	0.09024
	Design 3.3.2.2	**Design 3.3.2.3**
Direct effects	0.05735	0.05730
Residual effects	0.06543	0.06537
	Design 3.3.3.2	**Design 3.3.3.3**
Direct effects	0.04713	0.04636
Residual effects	0.05232	0.05217

Designs 3.2.3, 3.3.1.3 and 3.3.2.3 have the following sequence of matrix M.

$$\begin{bmatrix} 0 & 1 & 3 & 3 & 1 \\ 1 & 0 & 1 & 3 & 3 \\ 3 & 1 & 0 & 1 & 3 \\ 3 & 3 & 1 & 0 & 1 \\ 1 & 3 & 3 & 1 & 0 \end{bmatrix} \qquad \begin{bmatrix} 0 & 1 & 3 & 2 & 2 & 3 & 1 \\ 1 & 0 & 1 & 3 & 2 & 2 & 3 \\ 3 & 1 & 0 & 1 & 3 & 2 & 2 \\ 2 & 3 & 1 & 0 & 1 & 3 & 2 \\ 2 & 2 & 3 & 1 & 0 & 1 & 3 \\ 3 & 2 & 2 & 3 & 1 & 0 & 1 \\ 1 & 3 & 2 & 2 & 3 & 1 & 0 \end{bmatrix}$$

$$\begin{bmatrix} 0 & 1 & 3 & 2 & 2 & 2 & 2 & 3 & 1 \\ 1 & 0 & 1 & 3 & 2 & 2 & 2 & 2 & 3 \\ 3 & 1 & 0 & 1 & 3 & 2 & 2 & 2 & 2 \\ 2 & 3 & 1 & 0 & 1 & 3 & 2 & 2 & 2 \\ 2 & 2 & 3 & 1 & 0 & 1 & 3 & 2 & 2 \\ 2 & 2 & 2 & 3 & 1 & 0 & 1 & 3 & 2 \\ 2 & 2 & 2 & 2 & 3 & 1 & 0 & 1 & 3 \\ 3 & 2 & 2 & 2 & 2 & 3 & 1 & 0 & 1 \\ 1 & 3 & 2 & 2 & 2 & 2 & 3 & 1 & 0 \end{bmatrix} \qquad (3.4.1)$$

The M matrix of design 3.3.3.3 also follows a similar pattern. Equation (2.2.1.5) in the previous chapter indicates that change-over designs in which there is equal replication of treatments within periods as well as equal replication within experimental units can be assessed or classified in terms of matrix M only. The designs with the sequence of matrix M given in (3.4.1) are easily obtainable.

3.4.1 Construction of designs for 2 by k factorial experiments plus control treatment

The total number of treatments is $t = 2k + 1$. The designs which give the sequence of M matrices in (3.4.1) are derived from two latin squares whose $2(t-1)$ differences (mod t) of successive rows of both the squares have both the members of the set $(1, t-1)$ appearing

once, both the members of the set $(2,t-2)$ appearing thrice, and both the members of the sets $(3,t-3)$, $(4,t-4)$ up to $[(t-1)/2,(t+1)/2]$ appearing twice.

Alternatively, the designs can be derived from two squares, where the second square is formed by reversing the order of the rows of the first square. The $t-1$ differences (mod t) of successive rows of the initial square follow the rule:-

One difference comes from the set $(1,t-1)$

Three differences come from the set $(2,t-2)$

Two differences come from each of the sets $(3,t-3)$, $(4,t-4)$ up to $((t-1)/2,(t+1)/2)$.

A method of construction using this second option is offered below.

Construction of the initial square of the design

Case (i): $(t-1)/2$ is an even number

(a) Construct an initial column whose odd-numbered elements are 0, 2, 4,..., $(t-1)/2$, $[(t-1)/2]-1$, $[(t-1)/2]-3$, $[(t-1)/2]-5$, ..., 1 and whose even numbered elements are $t-2$, $t-4$, ..., $(t+1)/2$, $[(t+1)/2]+1$, $[(t+1)/2]+3$, $[(t+1)/2]+5$, ..., $t-1$

(b) Generate $(t-1)$ further columns by cyclic development (mod t) of the first column

Lemma 1

A square constructed from the above method has the differences between successive rows consisting of 2, 3, ..., $t-1$. The difference 2 appears twice while each of the rest appear once.

Proof

The initial column of the square is given by 0, $t-2$, 2, $t-4$, 4, $t-6$, 6, ..., $(t+1)/2$, $(t-1)/2$, $[(t+1)/2]+1$, $[(t-1)/2]-1$, $[(t+1)/2]+3$, $[(t-1)/2]-3$, $[(t+1)/2]+5$, $[(t-1)/2]-5$, ..., $t-3$, 3, $t-1$, 1. The differences between successive columns (mod t) are therefore $t-2$, 4,

t -6, 8, t -10, 12, ..., 3, t -1, 2, t -3, 5, t -5, 9, t - 7, ..., 6, t -4, 2. Thus the differences consist of 2, 3, ..., t -1, with 2 repeated and each of the rest appearing once.

Example

Let number of treatments t =9. The odd-numbered elements of the initial column of the first square are 0, 2, 4, 3, 1. The even-numbered elements of the initial column of the first square are 7, 5, 6, 8. The design is as shown below. The rows represent periods and the columns represent experimental units.

0	1	2	3	4	5	6	7	8
7	8	0	1	2	3	4	5	6
2	3	4	5	6	7	8	0	1
5	6	7	8	0	1	2	3	4
4	5	6	7	8	0	1	2	3
6	7	8	0	1	2	3	4	5
3	4	5	6	7	8	0	1	2
8	0	1	2	3	4	5	6	7
1	2	3	4	5	6	7	8	0

1	2	3	4	5	6	7	8	0
8	0	1	2	3	4	5	6	7
3	4	5	6	7	8	0	1	2
6	7	8	0	1	2	3	4	5
4	5	6	7	8	0	1	2	3
5	6	7	8	0	1	2	3	4
2	3	4	5	6	7	8	0	1
7	8	0	1	2	3	4	5	6
0	1	2	3	4	5	6	7	8

The M matrix for this design is the last one of the three matrices in (3.4.1)

Case (ii) (t -1)/2 is an odd number

(a) Construct an initial column whose odd-numbered elements are 0, 2, 4,..., [(t -1)/2] -1, (t -1)/2, [(t -1)/2] -2, [(t -1)/2] -4, ..., 1 and whose even numbered elements are t -2, t -4, ..., [(t +1)/2] +1, (t +1)/2, [(t +1)/2] +2, [(t +1)/2] +4, ..., t -1.

(b) Generate (t -1) further columns by cyclic development (mod t) of the first column

Lemma 2

A square constructed from the above method has the differences between successive rows consisting of 2, 3, ..., t - 1. The difference 2 appears twice while each of the rest appear once.

Proof

The initial column of the square is 0, t -2, 2, t -4, 4, t -6, 6, ..., [(t +1)/2]+1, [(t -1)/2]-1, (t +1)/2, (t -1)/2], [(t +1)/2]+2, [(t -1)/2]-2, [(t +1)/2]+4, [(t -1)/2]-4, ..., t -3, 3, t -1, 1. The differences between successive columns (mod t) are therefore t -2, 4, t -6, 8, t -10, 12, ..., t -3, 2, t -1, 3, t -5, 7, t - 9, ..., 6, t -4, 2. Thus the differences consist of 2, 3, ..., t -1, with 2 repeated and each of the rest appearing once.

Example

Let number of treatments t =7. The odd-numbered elements of the initial column of the first square are 0, 2, 3, 1. The even-numbered elements of the initial column of the first square are 5, 4, 6. The design is therefore as shown below. The rows represent periods and the columns represent experimental units.

0	1	2	3	4	5	6
5	6	0	1	2	3	4
2	3	4	5	6	0	1
4	5	6	0	1	2	3
3	4	5	6	0	1	2
6	0	1	2	3	4	5
1	2	3	4	5	6	0

1	2	3	4	5	6	0
6	0	1	2	3	4	5
3	4	5	6	0	1	2
4	5	6	0	1	2	3
2	3	4	5	6	0	1
5	6	0	1	2	3	4
0	1	2	3	4	5	6

The M matrix of the design is the second of the matrices in (3.4.1)

3.4.2 Designs for 2 by k experiment where n=t

The designs considered so far have the number of experimental units being twice the number of treatments in order to see how competitive they are with the Williams squares designs which are universally optimal. This section considers designs for the same kind of experiments as listed in 3.3 and the coding of the treatment combinations is the same as given at the end of section 3.2.

According to Hedayat and Afsarinejad (1978), the optimal design for which ***n=p=t*** would have its matrix $M = J_{t,t} - I_t$. As such an M cannot be attained by a cyclic construction for t odd (Williams, 1949), Russell (1991) has indicated that the next best choice for a cyclic construction would be a matrix whose first row has two zeros, one two and (t -3) ones. Russell has subsequently given a method of construction of $t \times t$ squares that yield this kind of M matrix. Any $t \times t$ square constructed by the method given in section 3.4.1 also yields this kind of M matrix.

Example

The first square of the design for t =7 given in the previous section yields the following matrix M.

$$M = \begin{bmatrix} 0 & 1 & 1 & 1 & 1 & 2 & 0 \\ 0 & 0 & 1 & 1 & 1 & 1 & 2 \\ 2 & 0 & 0 & 1 & 1 & 1 & 1 \\ 1 & 2 & 0 & 0 & 1 & 1 & 1 \\ 1 & 1 & 2 & 0 & 0 & 1 & 1 \\ 1 & 1 & 1 & 2 & 0 & 0 & 1 \\ 1 & 1 & 1 & 1 & 2 & 0 & 0 \end{bmatrix}$$

The method given by Russell and the one presented here both give the same total variance for the direct effects as well as for the residual effects. The variances given by these designs provide the lower bound for the class of designs that are constructed cyclically for which

$p=n=t$ where t is odd. For $2 \times k$ factorial experiments with a control treatment, no other designs that are developed cyclically have been found competitive enough, either in the estimation of the main effects or interaction effects, to warrant being mentioned here. It is therefore recommended that for situations where $p=n=t$, the above mentioned squares should be used.

3.5 Designs for 3 by k factorial experiments and a control treatment (k=3,5..)

These designs were considered for $t = (3k + 1)$ treatments, t periods and t experimental units. No set pattern was established for these designs. Designs for which each level of a given factor follows itself and every other level of the same factor nearly equal times provided better estimates of the contrasts of the main effects of the given factor than designs for which the levels of the given factor follows each other at frequencies that are far from equal. The coding of the treatments is as given below.

Treatment combination	Coding
Control treatment	0
a_1b_1	1
a_2b_1	2
a_3b_1	3
⋮	⋮
a_3b_k	$3k$

3.5.1 Designs for 3 by 3 factorial experiment plus a control

Design 3.5.1.1

This design is derived from a Williams square. The relationship of the levels of factor A are as shown below:

$a_1 \rightarrow a_1$=6	$a_1 \rightarrow a_2$=9	$a_3 \rightarrow a_1$=9
$a_2 \rightarrow a_2$=6	$a_2 \rightarrow a_1$=9	$a_2 \rightarrow a_3$=9
$a_3 \rightarrow a_3$=6	$a_1 \rightarrow a_3$=9	$a_3 \rightarrow a_2$=9

Design 3.5.1.2

The design is made up of a latin square. The initial column of the square is 0,9,7,3,8,5,2,6,1,4. The other columns are generated from the initial one by cyclic development (mod 10). The relationships of the levels of factor A are given below:

$a_1 \rightarrow a_1$=8	$a_1 \rightarrow a_2$=8	$a_3 \rightarrow a_1$=8
$a_2 \rightarrow a_2$=8	$a_2 \rightarrow a_1$=8	$a_2 \rightarrow a_3$=8
$a_3 \rightarrow a_3$=8	$a_1 \rightarrow a_3$=8	$a_3 \rightarrow a_2$=8

The variances (divided by σ^2) of the two designs are given in the following table:

Table 3.5.1.1: Variances (divided by σ^2) for designs 3.5.1.1 and 3.5.1.2

		Design 3.5.1.1	Design 3.5.1.2
Control	direct effect	0.1011	0.1075
	residual effect	0.1136	0.1207
Main effect of A	direct effect	0.1011	0.1009
	residual effect	0.1136	0.1134
Main effect of B	direct effect	0.1011	0.1178
	residual effect	0.1136	0.1324
Interaction AB	direct effect	0.1011	0.1054
	residual effect	0.1136	0.1184

Design 3.5.1.2 does give a minuscule improvement in the estimation of the main effects of factor A over the Williams squares design.

3.6 Choice of designs

In a factorial experiment with a control, the choice of the design is based upon on the importance of the effects that are to be estimated. Most of the designs provided in the previous section are constructed in such a way as to minimise the variances of the main effects of factor *A*. Williams squares designs do give the minimum variances in the interaction effects.

Designs that satisfy Kiefer's rule for optimality mentioned in section 1.3 are also optimal for any set of ***t-1*** set of orthonormal contrasts corresponding to the main effects and interactions of a factorial experiment. As has been mentioned earlier, the Williams designs have been shown to be universally optimal for the estimation of both direct and residual treatment effects. Let A be a $(t-1)\times t$ matrix representing ***t-1*** orthonormal treatment contrasts of a design involving t treatments. The Williams squares designs then provide a lower bound for the following:

1. $tr(AC_d^{-}A^T)$
2. $tr(AC_r^{-}A^T)$ (3.6.1)

where $C_d = C_{11} - C_{12}C_{22}^{-}C_{21}$, $C_r = C_{22} - C_{21}C_{11}^{-}C_{12}$ are respectively the coefficient matrices for estimating direct and residual effects of the treatments separately, $C_{11}, C_{12}, C_{21}, C_{22}$ are as given in equation (2.2.4) in chapter 2.

Expression (3.6.1) is independent of the choice of the ***t-1*** orthonormal treatment contrasts since $tr(AC_d^{-}A^T) = tr(C_d^{-}A^TA) = tr(C_d^{-}[I_t - \frac{1}{t}J_{t,t}]) = tr(C_d^{-})$

This is due to the fact that $A^TA = I_t - \frac{1}{t}J_{t,t}$ from section (2.2.2) of chapter 2. It is assumed in this proof that the generalized inverse C_d^{-} is chosen such that $C_d^{-}1_t = 0_t$, where 1_t and 0_t are $t\times 1$ vector of ones and zeros respectively. However, as the least squares estimator of an

estimable contrast is invariant to the choice of generalized inverse (Searle (1971)), the result does not depend on the particular generalized inverse being used.

In an experiment in which all the effects are given equal emphasis, the Williams squares designs would be the best. In situations in which the main effects of factor A are of greatest importance, the designs given in the previous sections would then be more appropriate.

3.6.1 Strongly balanced designs

Definition: A change-over design d for t treatments p periods and n experimental units is said to be **strongly balanced** if the collection of ordered pairs $(d(i,j),d(i+1,j))$, $1 \le i \le p-1, 1 \le j \le n$, contains each ordered pair of treatments , distinct or not, the same number of times, say λ times. $d(i,j)$ is as defined in section 1.2 of chapter 1. For such designs, $\lambda = t^{-2}(p-1)n$.

Strongly balanced designs have direct and residual effects orthogonal as discussed in section 2.3 of the last chapter. **Strongly balanced uniform designs** are strongly balanced designs which have equal replication of treatments among periods as well as equal replication of treatments among experimental units. The designs given by Berenblut (1964) are strongly balanced uniform designs. Cheng and Wu (1980) have shown that these designs are universally optimal for the estimation of direct treatment effects as well as residual treatment effects for the class of designs for t treatments, p periods and n experimental units for which they exist. For a given factorial experiment with a control, these designs provide a balanced design for the levels of the different factors. That is, for any given factor, each level of the factor follows every other level of the same factor an equal number of times. For any two factors A and B, each level of factor A follows every other level of factor B an equal number of times, say "m ", and also each level of B follows each level of factor A "m " times. In other words, these designs are the best in the estimation of the main effects as well

as the interaction effects. The greatest shortcoming with these designs is that they are too large for practical use.

Designs that have the same property as the strongly balanced uniform designs are the **extra-period** change-over designs. In an experiment where there are t treatments, these designs have $t+1$ periods. The treatment applied on each experimental unit on the last period is the same as the one applied on the second last period. For these designs, the matrix M equals $kJ_{t,t}$, where k is some integer. The designs that are considered here are for t treatments, $p = t+1$ periods and $n = 2t$ experimental units. These designs are constructed in the same way as the Williams squares but have the last row repeated. The matrices for these designs are given below:-

$$D = 2(t+1)I_t \qquad \tilde{D} = 2tI_t$$

$$N_p = 2J_{t,t+1} \qquad \tilde{N}_p = 2\left[0_t, J_{t,t}\right]$$

$$N_u = \left[J_{t,t} + P_1, J_{t,t} + P_2\right] \qquad \tilde{N}_u = J_{t,2t}$$

where P_1, P_2 are $t \times t$ permutation matrices. Then

$$C_{11} = 2(t+1)I_t - \frac{4}{2t}J_{t,t+1}J_{t+1,t} - \frac{1}{t+1}\left[J_{t,t} + P_1, J_{t,t} + P_2\right]\begin{bmatrix} J_{t,t} + P_1^T \\ J_{t,t} + P_1^T \end{bmatrix}$$

$$+\frac{1}{2t(t+1)}\left[J_{t,t} + P_1, J_{t,t} + P_2\right]J_{2t,2t}\begin{bmatrix} J_{t,t} + P_1^T \\ J_{t,t} + P_1^T \end{bmatrix}$$

$$= 2(t+1)I_t - \frac{4(t+1)}{2t}J_{t,t} - \frac{2}{t+1}\left[(t+2)J_{t,t} + I_t\right] + \frac{4(t+1)^2}{2t(t+1)}J_{t,t}$$

$$= \frac{2t(t+2)}{(t+1)}I_t - \frac{2(t+2)}{t+1}J_{t,t}$$

$$C_{12} = 0$$

$$C_{22} = 2tI_t - \frac{4}{2t}\left[0_t, J_{t,t}\right]\begin{bmatrix} 0_t^T \\ J_{t,t} \end{bmatrix} - \frac{1}{t+1}J_{t,2t}J_{2t,t} + \frac{1}{2t(t+1)}J_{t,2t}J_{2t,2t}J_{2t,t}$$

$$= 2tI_t - \frac{4t}{2t}J_{t,t} - \frac{2t}{t+1}J_{t,t} + \frac{4t^2}{2t(t+1)}J_{t,t} = 2tI_t - 2J_{t,t}$$

From (3.6.1) the coefficient matrices for estimating direct effects and residual effects separately are then given by $C_d = C_{11}$ and $C_r = C_{22}$. These matrices are completely symmetric in the sense of Keifer. In order to prove that these designs are optimal in the estimation of both direct and residual effects in their class of designs for t treatments, $p = t+1$ periods and $n = 2t$ experimental units, it needs to be shown that these designs maximise trC_{11} and trC_{22}.

The lemma and Proof 2 below are given by Cheng and Wu (1980).

Lemma

For any positive integers s and t, the minimum of $\sum_{i=1}^{s} n_i^2$ subject to $\sum_{i=1}^{s} n_i = t$, where n_i's are non-negative integers, is obtained when $t - s\left[t/s\right]$ of the n_i's are equal to $\left[t/s\right]+1$ and the others are equal to $\left[t/s\right]$, where $\left[t/s\right]$ is the largest integer $\leq t/s$.

Proof 1: These designs maximise trC_{11}

$$trC_{11} = np - n^{-1}\sum_{i=1}^{t}\sum_{k=1}^{p} l_{ik}^2 - p^{-1}\sum_{i=1}^{t}\sum_{u=1}^{n} n_{iu}^2 + n^{-1}p^{-1}\sum_{i=1}^{t} r_i^2$$

$$= np - p^{-1}\sum_{i=1}^{t}\sum_{u=1}^{n} n_{iu}^2 - n^{-1}\left[\sum_{i=1}^{t}\sum_{k=1}^{p}\left(l_{ik} - p^{-1}r_i\right)^2 + \left\{p^{-1} - p^{-1}\right\}\sum_{i=1}^{t} r_i^2\right] \text{ since } r_i = \sum_{k=1}^{p} l_{ik}.$$ The designs described above have equal replication of treatments within each period. This implies that $l_{ik} = p^{-1}r_i$, so it suffices to show that these designs minimises $\sum_{i=1}^{t}\sum_{u=1}^{n} n_{iu}^2$. This summation is minimised when n_{iu}'s are as nearly equal as possible subject to the constraint that $\sum_{i=1}^{t} n_{iu} = p = t+1$. In the designs that are described, $t-1$ n_{iu}'s are equal to 1 and one n_{iu} is equal to 2, for all u. This implies that the designs described above are optimal in the estimation of direct effects.

Proof 2: These designs maximise the trC_{22}

$$trC_{22} = n(p-1) - n^{-1}\sum_{i=1}^{t}\sum_{k=1}^{p-1} l_{ik}^2 - p^{-1}\sum_{i=1}^{t}\sum_{u=1}^{n}\tilde{n}_{iu}^2 + n^{-1}p^{-1}\sum_{i=1}^{t}\tilde{r}_i^2$$

$$= n(p-1) - p^{-1}\sum_{i=1}^{t}\sum_{u=1}^{n}\tilde{n}_{iu}^2 - n^{-1}\left[\sum_{i=1}^{t}\sum_{k=1}^{p}\left(l_{ik} - (p-1)^{-1}\tilde{r}_i\right)^2 + \left\{(p-1)^{-1} - p^{-1}\right\}\sum_{i=1}^{t}\tilde{r}_i^2\right] \text{since}$$

$\tilde{r}_i = \sum_{k=1}^{p-1} l_{ik}$.

The designs described above have equal replication of treatments on the periods, therefore $\sum_{i=1}^{t}\sum_{k=1}^{p}\left(l_{ik} - (p-1)^{-1}\tilde{r}_i\right)^2 = 0$, so it suffices to show that the designs minimise $\sum_{i=1}^{t}\sum_{u=1}^{n}\tilde{n}_{iu}^2$ and $\sum_{i=1}^{t}\tilde{r}_i^2$. This follows immediately from the given lemma since $\sum_{i=1}^{t}\sum_{u=1}^{n}\tilde{n}_{iu} = n(p-1) = \sum_{i=1}^{t}\tilde{r}_i$,

$\tilde{n}_{iu}$ are all equal and

$|n_{iu} - n_{i'u'}| \leq 1$ for all $(i,u) \neq (i',u')$.

In conclusion, these designs are universally optimal for the estimation of direct effects as well as residual effects for the class of designs for t treatments, $p = t+1$ periods and $n = 2t$ experimental units.

Use of these designs for factorial experiments plus control

Strongly balanced designs have been found to be very suitable for factorial experiments with a control. In these designs, each level of each factor follows itself and every other level of the same factor an equal number of times. Also for any pair of factors, each level of one factor follows each level of the second factor an equal number of times and vice versa. Sections 3.2, 3.3 and 3.5 indicate that the efficiency in the estimation of the main effects of a given factor depends to some extent on how nearly equal the levels of the factor follow each other. These designs provide not only the lower bound of the total variances of the direct effects as

well as residual effects, but also provide the minimum bound for the estimation of the main effects and interaction effects of the factors (both direct and residual).

Example: 2 by 2 experiment plus control treatment

This design consists of the standard William squares and with an additional row which is a replicate of the second last row. The relationship of the various levels of the factors are shown below.

$a_1 \rightarrow a_1$ = 8 times	$a_1 \rightarrow b_1$ = 8 times
$a_1 \rightarrow a_2$ = 8 times	$a_1 \rightarrow b_2$ = 8 times
$a_2 \rightarrow a_1$ = 8 times	$a_2 \rightarrow b_1$ = 8 times
$a_2 \rightarrow a_2$ = 8 times	$a_2 \rightarrow b_2$ = 8 times
$b_1 \rightarrow b_1$ = 8 times	$b_1 \rightarrow a_1$ = 8 times
$b_1 \rightarrow b_2$ = 8 times	$b_1 \rightarrow a_2$ = 8 times
$b_2 \rightarrow b_1$ = 8 times	$b_2 \rightarrow a_1$ = 8 times
$b_2 \rightarrow b_2$ = 8 times	$b_2 \rightarrow a_2$ = 8 times

The variances (divided by σ^2) of this design are compared with the variances of a Williams square design of section 3.2

Table 3.6.1.1: Variances (divided by σ^2) for Williams square design and extra-period design

		Williams design	Extra-period design
Control vs treats	direct	0.106	0.086
	residual	0.139	0.100
main effects of A	direct	0.106	0.086
	residual	0.139	0.100
main effects of B	direct	0.106	0.086
	residual	0.139	0.100
Interaction AB	direct	0.106	0.086
	residual	0.139	0.100
Total variances	direct	0.424	0.344
	residual	0.556	0.400

In the above example, the total variances of the extra-period design (both direct and residual) are lower than $\frac{5}{6}$ of the total variances of Williams designs. In actual fact, the extra-period designs perform better than Williams square designs in all counts even after taking into account the extra period. The extra-period design gives a reduction in the total variance of 18.9 % and 28.1% for direct treatment effects and residual treatments effects respectively. Both reductions in variance are substantial. The use of extra-period designs over William squares designs results in a greater reduction in the variance of residual treatment effects than the reduction in variance of the direct treatment effects. The reduction in variance reduces as the designs become larger, ie the number of treatments becomes larger. No other designs with equal replication of treatments for t treatments, $p=t+1$ periods perform better than the extra period designs in the estimation of any of the effects either direct or residual. The extra period designs are better suited for factorial experiments with a control treatment than any other designs.

Chapter 4
Designs in which number of Periods is less than number of Treatments

In this chapter, a review of some of the previous work of change-over designs for factorial experiments is given. An investigation of change-over designs for factorial experiments with a control is given where the number of periods p is less that the number of treatments t. Optimal replication for the given contrasts is discussed.

4.1: Bricks in Change-over designs for factorial experiments

The concept of factorial structure, mentioned earlier, has been used as a basis for selecting change-over designs for use in factorial experiments. Generalized cyclic change-over designs are widely used for factorial experiments and have the factorial structure. Fletcher (1987) has devised a method of determining direct and residual canonical efficiency factors for generalized cyclic change-over designs. This method is used as a basis for selection of designs. The concept of bricks is also used in the construction of change-over designs for factorial experiments. This concept was developed by Jones (1985). A brick is a set of blocks too small to be used as a design by itself but, used in conjunction with other bricks, can form a design. A brick provides uncorrelated estimates of the treatment contrasts.

From equation (2.2.5) and section 2.4, the reduced normal equations for estimating direct effects are

$$C_d\hat{\tau} = q_1$$

If $u^T\tau$ is a treatment contrast of interest, then from (1.5.2) the variance of the contrast is

$$Var(u^T\tau) = u^T C_d^- u\sigma^2$$

where C_d^- is a generalized inverse of C_d and σ^2 is the variance of each observation.

Suppose C_d has eigenvectors $u_1, u_2, \cdots, u_h$ with corresponding non-zero eigenvalues $\lambda_1, \lambda_2, \cdots, \lambda_h$, $h \leq (t-1)$ where t is the number of treatments. Then a convenient generalized inverse for C_d is the Moore-Penrose inverse $C_d^* = \sum_{i=1}^{h} \frac{1}{\lambda_i} u_i u_i^T$.

Since u_i's are such that $u_i^T 1 = 0$, $u_i'^T u_i = 1$, $u_i^T u_j = 0$ for $i \neq j$, they are orthonormal contrasts. It therefore follows that

$$Var(u_i^T \tau) = \frac{u_i^T u_i}{\lambda_i} \sigma^2 = \frac{\sigma^2}{\lambda_i}$$

λ_i is referred to as the **effective replication** of the contrast u_i. $u_i^T \tau$'s are called the natural contrasts. The case of residual effects is given by replacing the C_d with C_r.

Definition of a brick and the optimality criterion

Suppose an experimenter wishes to obtain a design that provides uncorrelated estimates of the treatment contrasts $u_1, u_2, \cdots, u_h$. This can be achieved if these contrasts are eigenvectors of C_d, that is the natural contrasts. A **brick** may be defined as a design for which the number of periods p is less than t, which provides a C_d such that

$$C_d u_i = \lambda_i u_i \quad \lambda_i \neq 0 \text{ for all } i \qquad (4.1.1)$$

ie u_i is an eigenvector for C_d.

If for the given set of contrasts, B_1 is a brick for p periods and n_1 experimental units with effective replication $\lambda_{11}, \lambda_{12}, \cdots, \lambda_{1h}$ and B_2 is another brick for p periods and n_2 experimental units with effective replication $\lambda_{21}, \lambda_{22}, \cdots, \lambda_{2h}$, then the design formed by aggregating n_1 experimental units of B_1 and n_2 experimental units of B_2 also has $u_1, u_2, \cdots, u_h$ as natural contrasts with effective replications $(\lambda_{11} + \lambda_{21}), (\lambda_{12} + \lambda_{22}), \cdots, (\lambda_{1h} + \lambda_{2h})$.

Lewis, Fletcher and Matthews (1988) have provided a guide on how to combine bricks in such a manner as to get the most efficient designs for the case of generalized cyclic designs.

The search for bricks involves considering all the generalized cyclic change-over sets of sequences of the same size. A brick is found when a given set satisfies equation (4.1.1).

Factorial experiments with a control provide a problem in that there is no simplification in the calculation of efficiency factors as in the case of generalized cyclic change-over designs. Also, the contrasts involved in these experiments fail to fit as natural contrasts and therefore the concept of bricks is not useful in this case.

4.2: Change-over designs for factorial experiments with a control for p<t

The most frequent cases that arise in factorial experiments involve 2 factors of 2 levels each or one of 2 levels and another of 3 levels. This section will consider designs for these kind of experiments but with an additional treatment taken as the control treatment. The designs are for $n=t$ or $2t$ experimental units and $p=3$ or 4 periods. The experiment consists of factor A with 2 levels and factor B with 2 or 3 levels. The treatments are labelled in the same manner as indicated in (3.2.1). The first set of designs is chosen such as to minimise the variance of the main effects of factor A. The second set of designs is chosen such as to minimise the total variance of a given set of $t-1$ orthonomal contrasts. These designs are chosen from a complete enumeration of all possible designs that are generated cyclically for the case of $n=t$, and for all combinations of these designs for the case of $n=2t$.

2×2 factorial experiment + control treatment; $p=3$, $n=5$

The designs that have minimum variances for the estimation of the main effects of A give the following variances (divided by σ^2):

Table 4.2.1: Variances (divided by σ^2) for designs in which $p=3$ and $n=5$

Control	direct effect	3.500
	residual effect	6.500
Main effect of A	direct effect	0.900
	residual effect	1.500
Main effect of B	direct effect	6.100
	residual effect	11.500
Interaction AB	direct effect	3.500
	residual effect	6.500

The total variance for the direct effects for these designs is 14.000 σ^2. One of the designs which gives these variances has initial column given by 0,4,1. Another design has the initial column as 0,1,4.

The designs that have minimum total variance for the direct effects have their initial columns given by (0,1,2), (0,2,4), (0,3,1) and (0,4,3). The total variance for the direct effects in these designs is 8.000 σ^2. These designs also give the lowest total variance for the residual effects. The contrasts of the direct effects and the residual effects are described in section 3.2.

2×2 factorial experiment + control $p=4$, $n=5$

The designs that give minimum variance for the main effects of factor A have the following variances (divided by σ^2):

Table 4.2.2: Variances (divided by σ^2) for designs in which $p=4$ and $n=5$

Control	direct effect	0.3456
	residual effect	0.5105
Main effect of A	direct effect	0.2806
	residual effect	0.3691
Main effect of B	direct effect	0.4105
	residual effect	0.6518
Interaction AB	direct effect	0.3455
	residual effect	0.5105

The initial columns of the designs that yield these variances are 0,2,1,4 and 0,3,4,1. For direct effects, the total variance of these designs is $1.3822\,\sigma^2$.

The designs that give minimum total variance have initial columns given by (0,1,4,3), (0,2,3,1), (0,3,2,4) and (0,4,1,2). For direct effects, the total variance for these designs is $1.2986\,\sigma^2$. These designs also give the minimum total variance for the residual effects. The differences of consecutive periods (mod 5) of these designs consist of all possible combinations of 3 non-zero integers (mod 5).

2×2 factorial experiment + control $p=3$, $n=10$

The design which are best for the estimation of the main effects of A have the following variances (divided by σ^2):

Table 4.2.3: Variances (divided by σ^2) for designs in which ***p=3*** and ***n=10***

Control	direct effect	0.2763
	residual effect	0.4398
Main effect of A	direct effect	0.2018
	residual effect	0.3770
Main effect of B	direct effect	0.3508
	residual effect	0.5026
Interaction AB	direct effect	0.2763
	residual effect	0.4398

The designs are made up of two 3×5 arrays. The pairs of initial columns of the arrays that make up the designs are (0,1,4 and 0 2,1) and (0,3,4 and 0,4,1).The total variance for the estimation of the direct effects of these designs is $1.1052\,\sigma^2$.

There are 2 designs that give minimum variances for the direct effects. The total variance for the estimation of direct effects for these designs is $1.0125\,\sigma^2$. One of the design is given below. The rows represent periods and the columns represent experimental units.

$$\begin{array}{ccccc} 0 & 1 & 2 & 3 & 4 \\ 1 & 2 & 3 & 4 & 0 \\ 3 & 4 & 0 & 1 & 2 \end{array} \qquad \begin{array}{ccccc} 0 & 1 & 2 & 3 & 4 \\ 4 & 0 & 1 & 2 & 3 \\ 2 & 3 & 4 & 0 & 1 \end{array} \qquad (4.2.1)$$

Another design that gives the same results has 0,2,1 and 0,3,4 as its initial columns.

The matrix *M* of these designs is as follows:

$$M = \begin{bmatrix} 0 & 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 & 1 \\ 1 & 1 & 0 & 1 & 1 \\ 1 & 1 & 1 & 0 & 1 \\ 1 & 1 & 1 & 1 & 0 \end{bmatrix}$$

The designs which give minimum variance for the estimation of main effects of factor A also have this kind of M matrix.

2×2 factorial experiment + control ***p=4, n=10***

The designs which are best for the estimation of the main effects of A give the following minimum variances (divided by σ^2):

Table 4.2.4: Variances (divided by σ^2) for designs in which ***p=4*** and ***n=10***

Control	direct effect	0.1629
	residual effect	0.2398
Main effect of A	direct effect	0.1371
	residual effect	0.1801
Main effect of B	direct effect	0.1886
	residual effect	0.2994
Interaction AB	direct effect	0.1629
	residual effect	0.2398

There are 4 designs that give these variances. They are formed by two 4×5 arrays . The pairs of the initial columns of these designs are (0,1,4,2 and 0,4,1,3), (0,2,1,3 and 0,3,4,2), (0,2,1,4 and 0,3,2,4) and (0,2,1,4 and 0,3,4,1). The total variance for the direct effects for these designs is $0.6515\,\sigma^2$.

There are 4 designs that give minimum total variance for the direct effects. These designs also give minimum total variance for the residual effects. They are made up of the following pairs of initial columns:- (0,1,2,4 and 0,4,3,1), (0,1,4,3 and 0,4,1,2), (0,2,3,1 and 0,3,2,4) and (0,2,4,3 and 0,3,1,2). These designs have total variance of the direct effects equal to $0.5960\,\sigma^2$.

2×3 factorial experiment + control $p=3$, $n=7$

The designs that give the minimum variance for the estimation of the main effects of A also give the minimum total variance for the direct effects. The variances (divided by σ^2) for this design are as follows:

Table 4.2.5: Variances (divided by σ^2) for designs in which $p=3$ and $n=7$

Control	direct effect	1.1667
	residual effect	2.3333
Main effect of A	direct effect	0.7857
	residual effect	1.0000
Main effect of B	direct effect	1.5238
	residual effect	3.5834
Interaction AB	direct effect	1.0000
	residual effect	1.7500

The total variance for the direct effects for this design is $4.4762\,\sigma^2$. The designs that give these variances have 0,1,5 and 0,6,2 as their initial columns. These designs also give the lowest total variance for residual effects.

2×3 factorial experiment + control $p=4$, $n=7$

The designs which are best for the estimation of the main effects of A have the following variances (divided by σ^2):

Table 4.2.6: Variances (divided by σ^2) for designs in which ***p=4*** and ***n=7***

Control	direct effect	0.3915
	residual effect	0.5620
Main effect of A	direct effect	0.2807
	residual effect	0.3804
Main effect of B	direct effect	0.4529
	residual effect	0.6800
Interaction AB	direct effect	0.3854
	residual effect	0.5349

The total variance of the direct effects is 1.5105 σ^2. The initial columns of these designs are 0,2,1,6 and 0,5,6,1.

Designs that give minimum total variance of the direct effects have the following initial columns (0,3,4,1) and (0,4,3,6). The total variance of the direct effects for these designs is 1.340 σ^2. These designs also give minimum total variance for the residual effects.

2×3 factorial experiment + control ***p=3, n=14***

The design which is best for the estimation of the main effects of A has the following variances (divided by σ^2):

Table 4.2.7: Variances (divided by σ^2) for designs in which ***p=3*** and ***n=14***

Control	direct effect	0.4334
	residual effect	0.6702
Main effect of A	direct effect	0.1960
	residual effect	0.2929
Main effect of B	direct effect	0.6551
	residual effect	0.9846
Interaction AB	direct effect	0.3305
	residual effect	0.5444

The total variance of the direct effects is $1.6150\,\sigma^2$. The design is formed by two 3×7 arrays whose initial columns are 0,1,6 and 0,6,1.

Designs which give minimum total variance for the direct effects are formed from the following pairs of initial columns :- (0,1,3 and 0,6,2) and (0,1,5 and 0,1,4). The total variance for the direct effects is $1.0655\,\sigma^2$.

2×3 factorial experiment + control ***p=4, n=14***

The designs which are best for the estimation of the main effects of A have the following variances (divided by σ^2):

Table 4.2.8: Variances (divided by σ^2) for designs in which ***p=4*** and ***n=14***

Control	direct effect	0.1950
	residual effect	0.2733
Main effect of A	direct effect	0.1335
	residual effect	0.2028
Main effect of B	direct effect	0.2419
	residual effect	0.3460
Interaction AB	direct effect	0.1788
	residual effect	0.2360

The total variance of the direct effects is $0.7492\,\sigma^2$. The designs that gives these variances are formed by two 4×7 arrays. The pairs of the initial columns of the designs are (0,2,6,3 and 0,5,1,4) and (0,3,1,4 and 0,4,6,3).

The design which gives the minimum total variance of the direct effects is given below. The rows represent the periods and the columns represent the experimental units.

0	1	2	3	4	5	6		0	1	2	3	4	5	6
1	2	3	4	5	6	0		6	0	1	2	3	4	5
3	4	5	6	0	1	2		4	5	6	0	1	2	3
6	0	1	2	3	4	5		1	2	3	4	5	6	0

(4.2.2)

The total variance of the direct effects is $0.6262\,\sigma^2$. The matrix M for this design is as follows :-

$$\begin{bmatrix} 0 & 1 & 1 & 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 & 1 & 1 & 1 \\ 1 & 1 & 0 & 1 & 1 & 1 & 1 \\ 1 & 1 & 1 & 0 & 1 & 1 & 1 \\ 1 & 1 & 1 & 1 & 0 & 1 & 1 \\ 1 & 1 & 1 & 1 & 1 & 0 & 1 \\ 1 & 1 & 1 & 1 & 1 & 1 & 0 \end{bmatrix}$$

Each treatment in this design follows every other treatment once.

The designs that give minimum variances are in most cases not unique. There is no distinguishable feature that can be described in the designs that give the minimum of the total variance for the direct effects nor for designs that give minimum variances for the direct effects of the main effects of A . A common feature has been established for design (4.2.1) and design (4.2.2). Both of these designs give the minimum of the total variance of the direct effects for the number of periods and experimental units in which they belong. Designs (4.2.1) and (4.2.2) both have each treatment following every other treatment once. The next section looks further at the designs for which $p<t$ and have each treatment following every other treatment once.

4.3 Designs for which p<t and matrix $M = J_{t,t} - I_t$

The designs considered here are for $2 \times k$ ($k=2,3,4..$) factorial experiments plus a control treatment. The number of treatments is given by $t=2k+1$ and the number of periods is given by $p = (t+1)/2$ and the number of experimental units is given by $n = 2t$.The designs are

constructed from two $p \times n$ arrays. The matrices in equations (2.2.3) simplify for these designs to the form:-

$$D = 2pI_t \qquad \tilde{D} = (2p-1)I_t$$
$$N_p = 2J_{t,p} \qquad \tilde{N}_p = 2\left[0_t, J_{t,p-1}\right]$$

It follows therefore that

$$C_{11} = (t+1)I_t - \frac{2}{t+1}N_u N_u^T$$
$$C_{12} = (J_{t,t} - I_t) - \frac{2}{t+1}N_u \tilde{N}_u^T \qquad (4.3.1)$$
$$C_{22} = (t-1)I_t - \frac{2(t-1)}{t(t+1)}J_{t,t} - \frac{2}{t+1}\tilde{N}_u \tilde{N}_u^T$$

The matrices C_{11}, C_{12} and C_{22} are dependent on the matrices $N_u N_u^T$, $N_u \tilde{N}_u^T$ and $\tilde{N}_u \tilde{N}_u^T$ to have the complete symmetry as defined by Kiefer (see section 1.3). Subsequently, the matrices C_d and C_r are also dependent on the same matrices to have complete symmetry. This can only occur if ***t-1*** is divisible by ***p(p-1)*** . Since $p = (t+1)/2$, the condition becomes if ***t+1*** is divisible by 4. The condition further simplifies to the requirement that k be odd. For each array, the following differences (mod t) are considered:

$x_i - x_j$ where $i \neq j = 1, \cdots, p$ (4.3.2)

x_i, x_j represents the digits in the i th and j th positions of the initial column.

From (2.2.3)

$$N_u N_u^T = \begin{bmatrix} \sum_{u=1}^{n} n_{0u}^2 & \sum_{u=1}^{n} n_{0u}n_{1u} & \sum_{u=1}^{n} n_{0u}n_{2u} & \cdots & \sum_{u=1}^{n} n_{0u}n_{(t-1)u} \\ \sum_{u=1}^{n} n_{1u}n_{0u} & \sum_{u=1}^{n} n_{1u}^2 & \sum_{u=1}^{n} n_{1u}n_{2u} & \cdots & \sum_{u=1}^{n} n_{1u}n_{(t-1)u} \\ \vdots & \vdots & \vdots & & \vdots \\ \sum_{u=1}^{n} n_{(t-1)u}n_{0u} & \sum_{u=1}^{n} n_{(t-1)u}n_{1u} & \cdots & \sum_{u=1}^{n} n_{(t-1)u}n_{(t-2)u} & \sum_{u=1}^{n} n_{(t-1)u}^2 \end{bmatrix}$$

where $\sum_{u=1}^{n} n_{iu}n_{ju} = \sum_{u=1}^{n} n_{ju}n_{iu} = \lambda_v$ is the number of times treatments i and j are applied to the same experimental unit, $v = i - j \pmod t$ $v = 0,1,...,(t-1)$. This is due to the fact that the design is generated cyclically. It follows that

$$N_u N_u^T = \begin{bmatrix} \lambda_0 & \lambda_1 & \lambda_2 & \cdots & \lambda_{(t-1)} \\ \lambda_{(t-1)} & \lambda_0 & \lambda_1 & \cdots & \lambda_{(t-2)} \\ \vdots & \vdots & & & \vdots \\ \lambda_1 & \lambda_2 & \cdots & \lambda_{(t-1)} & \lambda_0 \end{bmatrix} \qquad (4.3.3)$$

This arrangement has a one to one correspondence with a cyclic incomplete block design for t treatments, $b=t$ blocks where the size of the blocks is $s=p$. $N_u N_u^T$ is then the concurrence matrix of the cyclic incomplete block design. From John (1987) it can be inferred that the values of the off-diagonal elements of the concurrence matrix of a cyclic incomplete block design are

λ_v =number of times the difference v occurs in the set (4.3.2), v =1,2..,t-1.

Suppose all the off-diagonal elements of (4.3.3) are equal to λ. Then the situation becomes similar to a balanced incomplete block design of t treatments, $b=t$ blocks of size $s=p$. Each treatment is replicated $r=p$ times and a pair of treatments occur together in the same block λ times. From the relation $r(s-1) = \lambda(t-1)$ and the values given above for p, the value of λ is $(t+1)/4$. It therefore follows that $N_u N_u^T$ is completely symmetric if the differences described in (4.3.2) on each array have the digits 1,...,t-1 appearing a total of $(t+1)/4$ times.

For the symmetry of $\tilde{N}_u \tilde{N}_u^T$ to be determined, the following differences (mod t) are considered for each array

$x_i - x_j$ where $i \neq j = 1,\cdots,p-1$ (4.3.4)

This situation is analogous to considering the values of the concurrence matrix of a cyclic block design with parameters t treatments, $b=t$ blocks each of size $p-1$. Following the same argument as the one presented above, it follows that $\tilde{N}_u\tilde{N}_u^T$ is completely symmetric if the differences described in (4.3.4) on each array have the digits $1,\ldots,t-1$ each appearing a total of $([t+1]/4)-1$ times.

Consider a design for which both $N_uN_u^T$ and $\tilde{N}_u\tilde{N}_u^T$ are both completely symmetric. The matrix $N_u\tilde{N}_u^T$ is given below :-

$$N_u\tilde{N}_u^T = \begin{bmatrix} \sum_{u=1}^{n} n_{0u}\tilde{n}_{0u} & \sum_{u=1}^{n} n_{0u}\tilde{n}_{1u} & \sum_{u=1}^{n} n_{0u}\tilde{n}_{2u} & \cdots & \sum_{u=1}^{n} n_{0u}\tilde{n}_{(t-1)u} \\ \sum_{u=1}^{n} n_{1u}\tilde{n}_{0u} & \sum_{u=1}^{n} n_{1u}\tilde{n}_{1u} & \sum_{u=1}^{n} n_{1u}\tilde{n}_{2u} & \cdots & \sum_{u=1}^{n} n_{1u}\tilde{n}_{(t-1)u} \\ \vdots & \vdots & \vdots & & \vdots \\ \sum_{u=1}^{n} n_{(t-1)u}\tilde{n}_{0u} & \sum_{u=1}^{n} n_{(t-1)u}\tilde{n}_{1u} & \cdots & \sum_{u=1}^{n} n_{(t-1)u}\tilde{n}_{(t-2)u} & \sum_{u=1}^{n} n_{(t-1)u}\tilde{n}_{(t-1)u} \end{bmatrix}$$

The sum $\sum_{u=1}^{n} n_{iu}\tilde{n}_{ju}$ is the number of times treatment j is applied in any of the first $(p-1)$ periods in the same experimental unit in which treatment i is applied in any of the periods. It follows that $\sum_{u=1}^{n} n_{iu}\tilde{n}_{iu} = 2(p-1) = t-1$ $i = 0,1,\ldots,t-1$. In the design, treatment i appears in the first $(p-1)$ periods a total of $(t-1)$ times. For $\sum_{u=1}^{n} n_{iu}\tilde{n}_{ju}$ $(i \neq j)$ to be all equal, the other $(t-1)$ treatments should each appear in the last period once. This implies that $\sum_{u=1}^{n} n_{iu}\tilde{n}_{ju}$ $(i \neq j)$ are all equal if the totality of the differences (mod t) $x_i - x_p$ $j = 1,\cdots,p-1$ of the initial columns of both arrays should contain each of the differences $1,\ldots,t-1$ once. This is because the design is made up of two arrays each of which are developed cyclically.

With the above conditions fulfilled, these matrices become

$$N_u N_u^T = \frac{t+1}{2}\left(I_t + J_{t,t}\right)$$

$$N_u \tilde{N}_u^T = \frac{t-1}{2}\left(I_t + J_{t,t}\right)$$

$$\tilde{N}_u \tilde{N}_u^T = \frac{t+1}{2} I_t + \frac{t-3}{2} J_{t,t}$$

The matrices in (4.3.1) then simplify to

$$C_{11} = t(I_t - \frac{1}{t} J_{t,t})$$

$$C_{12} = -\frac{2t}{t+1}(I_t - \frac{1}{t} J_{t,t})$$

$$C_{22} = (t-2)\left(I_t - \frac{1}{t} J_{t,t}\right)$$

One generalized inverse of C_{22} is $\frac{I_t}{t-2}$ since $\left(I_t - \frac{1}{t} J_{t,t}\right)\frac{J_{t,t}}{t} = \frac{J_{t,t}}{t} - \frac{J_{t,t}}{t} = 0,$

hence it follows that

$$C_d = C_{11} - C_{12} C_{22}^{-} C_{21} = t\left(I_t - \frac{1}{t} J_{t,t}\right) - \left(\frac{2t}{t+1}\right)^2 \left(I_t - \frac{1}{t} J_{t,t}\right)^2 \left(\frac{I_t}{t-2}\right)$$

$$= \left\{ t - \frac{4t^2}{(t+1)^2 (t-2)} \right\} \left(I_t - \frac{1}{t} J_{t,t}\right) \qquad (4.3.5)$$

which is completely symmetric.

One generalized inverse for C_{11} is $\frac{1}{t} I_t$, so the reduced coefficient matrix for the residual effect is given by

$$C_r = C_{22} - C_{21} C_{11}^{-} C_{12} = (t-2)\left(I_t - \frac{1}{t} J_{t,t}\right) - \left(\frac{2t}{t+1}\right)^2 \left(I_t - \frac{1}{t} J_{t,t}\right)^2 \frac{I_t}{t}$$

$$= \left\{ (t-2) - \frac{4t}{(t+1)^2} \right\} \left(I_t - \frac{1}{t} J_{t,t}\right) \qquad (4.3.6)$$

which is also completely symmetric.

These designs are therefore universally optimal (see section 1.3) for both direct effects and residual effects for designs for ***t=2k+1*** , ***p=(t+1)/2*** and ***n=2t*** . For factorial experiments with a control, these designs would give the minimum total variances for the direct effects as well as for the residual effects.

Example 1

Consider the design for $t=7$. The initial column of the first array is 0,1,3,6 and the initial column of the second array is 0,6,4,1.

The differences (mod 7) of the initial columns of the first square and second square are respectively:

$$\begin{matrix} * & 1 & 3 & 6 \\ 6 & * & 2 & 5 \\ 4 & 5 & * & 3 \\ 1 & 2 & 4 & * \end{matrix} \quad \text{and} \quad \begin{matrix} * & 6 & 4 & 1 \\ 1 & * & 5 & 2 \\ 3 & 2 & * & 4 \\ 6 & 5 & 3 & * \end{matrix}$$

where the ij th entry represents $x_j - x_i$ for all $i \neq j$.

This design fulfils the conditions of symmetry and therefore the reduced coefficient matrices for estimating direct and residual effects separately take the form (4.3.5) and (4.3.6) respectively:

Example 2

Consider the design for $t=11$. The initial column of the first array is 0,1,5,10,8,7 and the initial column of the second array is 0,6,3,10,1,4. The differences (mod 11) of the initial columns of the first and second square are respectively:

$$\begin{matrix} * & 1 & 5 & 10 & 8 & 7 \\ 10 & * & 4 & 9 & 7 & 6 \\ 6 & 7 & * & 5 & 3 & 2 \\ 1 & 2 & 6 & * & 9 & 8 \\ 3 & 4 & 8 & 2 & * & 10 \\ 4 & 5 & 9 & 3 & 1 & * \end{matrix} \quad \text{and} \quad \begin{matrix} * & 6 & 3 & 10 & 1 & 4 \\ 5 & * & 8 & 4 & 6 & 9 \\ 8 & 3 & * & 7 & 9 & 1 \\ 1 & 7 & 4 & * & 2 & 5 \\ 10 & 5 & 2 & 9 & * & 3 \\ 7 & 2 & 10 & 6 & 8 & * \end{matrix}$$

where the ij th entry represents $x_j - x_i$ for all $i \neq j$.

Example 3

Consider the design for $t = 19$. The initial column of the first array is 0, 10, 11, 14, 1, 18, 3, 17, 5, 13 and the initial column of the second array is 0, 9, 8, 5, 18, 1, 16, 2, 14, 6. The differences (mod 19) of the first and second array are respectively:

*	10	11	14	1	18	3	17	5	13
9	*	1	4	10	8	12	7	14	3
8	18	*	3	9	7	11	6	13	2
5	15	16	*	6	4	8	3	10	18
18	9	10	13	*	17	2	16	4	12
1	11	12	15	2	*	4	18	6	14
16	7	8	11	17	15	*	14	2	10
2	12	13	16	3	1	5	*	7	15
14	5	6	9	15	13	17	12	*	8
6	16	17	1	7	5	9	4	11	*

and

*	9	8	5	18	1	16	2	14	6
10	*	18	15	9	11	7	12	5	16
11	1	*	16	10	12	8	13	6	17
14	4	3	*	13	15	11	16	9	1
1	10	9	6	*	2	17	3	15	7
18	8	7	4	17	*	15	1	13	5
3	12	11	8	2	4	*	5	17	9
17	7	6	3	16	18	14	*	12	4
5	14	13	10	4	6	2	7	*	11
13	3	2	18	12	14	10	15	8	*

where the ij th entry represents $x_j - x_i$ for all $i \neq j$

This design fulfils the conditions of symmetry and therefore the reduced coefficient matrices for estimating the direct effects and the residual effects separately take the form given in (4.2.4) and (4.3.5) respectively.

For $t = 15$, the author has been unable to find a cyclic balanced incomplete block design of parameters $b = 15$, $s = 8$, $\lambda = 4$, which can reduce to a cyclic BIB design (of parameters $t = 15$, $b = 15$, $s = 7$, $\lambda = 3$) by removing a plot from each block. It has therefore not been possible to construct a change-over design for $t = 15$ of the desired form.

The designs having the matrices C_d and C_r in the form given in (4.3.5) and (4.3.6) respectively minimise the total of the variances of the contrasts of both the main effects and the residual effects for the class of designs for which $t=2k+1$, $p=(t+1)/2$ and $n=2t$. To illustrate this, compare the following designs for $t=7$. The rows represent the periods and the columns represent the experimental units.

Design 4.3.1

0	1	2	3	4	5	6		0	1	2	3	4	5	6
1	2	3	4	5	6	0		6	0	1	2	3	4	5
3	4	5	6	0	1	2		4	5	6	0	1	2	3
6	0	1	2	3	4	5		1	2	3	4	5	6	0

This design is the one given in example 1 above. The C_d and C_r matrices for this design are completely symmetric.

Design 4.3.2

0	1	2	3	4	5	6		6	0	1	2	3	4	5
1	2	3	4	5	6	0		3	4	5	6	0	1	2
3	4	5	6	0	1	2		1	2	3	4	5	6	0
6	0	1	2	3	4	5		0	1	2	3	4	5	6

In this design, the second array is formed by reversing the order of the rows of the first array. The C_d and C_r matrices for this design are not completely symmetric.

Both designs 4.3.1 and 4.3.2 have their matrix M in the form $M = J_{t,t} - I_t$. The variances (divided by σ^2) of the designs are shown below:

Table 4.3.1: Variances (divided by σ^2) for designs 4.3.1 and 4.3.2

		Design 4.3.1	Design 4.3.2
Control	direct effect	0.157	0.158
	residual effect	0.219	0.226
Main effect of A	direct effect	0.157	0.167
	residual effect	0.219	0.272
Main effect of B	direct effect	0.157	0.153
	residual effect	0.219	0.200
Interaction AB	direct effect	0.157	0.159
	residual effect	0.219	0.228

Design 4.3.1 gives better variances than design 4.3.2 for both direct and residual effects. This is despite the fact that both designs have the same matrix M. From table 4.3.1, the reduction in the total variance of the direct treatment effects is 1.4% and the reduction in the total variance of the residual treatment effects is 5.4%. With all contrasts of equal interest, the designs that give information matrices of the form in (4.3.4) and (4.3.5) are the best for $2 \times k$ factorial experiment and a control treatment where k = 3,5,7 etc.

In this section, the aim has been to find universally optimal designs without regard to applicability. The restriction placed on the values of p and n may reduce the suitability of these designs.

4.4 Optimal replication for a given set of contrasts

Jones and Eccleston (1980) developed exchange and interchange procedures to search for optimal block designs. For a given set of treatment contrasts, the procedures give a design

which minimises the sum of weighted variances of a set of treatment contrasts of interest. The exchange procedure involves the exchange of observations resulting in the optimal replication for a set of weighted treatment contrasts. The interchange procedure is employed to obtain an optimal assignment of treatments to blocks. The same kind of procedures have been developed, by the same workers, for row-column designs.

In the present research, the exchange procedure has been employed to find the optimal replication for a set of orthonormal treatment contrasts for a factorial experiment and a control for a row-column arrangement. The experiment consists of two factors, factor A of two levels and factor B of levels 2,3, etc. The labelling of the treatments is as given in (3.2.1). The procedure gives equal replication for all treatments when the weight on all the contrasts is the same.

The situation considered here is for t treatments, $p=t$ periods and $n=2t$ experimental units. When enough weight is given to the treatment contrasts that represent the main effects as opposed to the interaction effects and the contrast representing the control treatment, the optimal replication of the control treatment is $t+1$ and the replication of the rest of the treatments is $2t+1$. Starting with a pair of Williams squares or any pair of squares that give the same M matrix as the Williams squares, the control treatment is replaced once by each of the other treatments in order to result in the optimal replication. With the exception of the interaction effects, the resulting design gives lower variances for both direct and residual effects than all the designs for which there is equal replication of treatments.

The following method of replacement of the control treatment gives designs with low total variance of the residual effects. Starting with a pair of $t \times t$ Williams squares (t is odd) perform the following replacements:

for the first square, replace the control treatment appearing in the (i,j)th position by the treatment appearing in the $(i-1,j)$th position $[i = 3, 5, 7,..., t\,;\, j = 2, 3,..., (t+1)/2]$

for the second square, replace the control treatment appearing in the (i,j)th position by the treatment appearing in the $(i-1,j)$th position $[i = 3, 5, 7,..., t\,;\, j = 1, 3,..., (t-1)/2]$.

Example

For a 2×2 factorial experiment with a control for 10 experimental units, the optimal replication of the control is 6 times and the optimal replication of all other treatments is 11 times. The design considered below is derived by the method given above. The rows represent periods and the columns represent experimental units

Design 1

0	1	2	3	4		3	4	0	1	2
1	2	3	4	0		2	3	4	0	1
4	2	1	2	3		4	3	1	2	3
2	3	4	0	1		1	2	3	4	0
3	4	4	1	2		1	1	2	3	4

The relationship of the different levels of the factors are shown below.

$a_1 \rightarrow a_1 = 6$ $\qquad$ $b_1 \rightarrow b_1 = 6$

$a_1 \rightarrow a_2 = 9$ $\qquad$ $b_1 \rightarrow b_2 = 9$

$a_2 \rightarrow a_1 = 9$ $\qquad$ $b_2 \rightarrow b_1 = 9$

$a_2 \rightarrow a_2 = 6$ $\qquad$ $b_2 \rightarrow b_2 = 6$

Design 2

The differences (mod 5) between consecutive digits of the initial columns of the 2 squares below consist of each positive number (mod 5) appearing twice. With equal replication of treatments, these squares would have each treatment following every other treatment twice.

The treatments 1,2,3,4 have each replaced the treatment 0 once. This design has not been constructed using the method given in the previous page, but has been given as an example of the general procedure of obtaining optimal replication.

0	1	2	3	4		2	3	4	0	1
1	2	3	4	4		4	0	1	2	3
3	4	3	1	2		3	4	1	1	2
4	0	1	2	3		1	2	3	4	2
2	3	4	0	1		0	1	2	3	4

The relationship of the different levels of the factors are as follows:

$a_1 \rightarrow a_1 = 8$ $\qquad$ $b_1 \rightarrow b_1 = 6$

$a_1 \rightarrow a_2 = 8$ $\qquad$ $b_1 \rightarrow b_2 = 9$

$a_2 \rightarrow a_1 = 8$ $\qquad$ $b_2 \rightarrow b_1 = 9$

$a_2 \rightarrow a_2 = 8$ $\qquad$ $b_2 \rightarrow b_2 = 6$

The variances (divided by σ^2) of the two designs follow:

Table 4.4.1: Variances (divided by σ^2) for designs 1 and 2

		Design 1	Design 2
Control	direct effect	0.1936	0.1849
	residual effect	0.1982	0.2876
Main effect of A	direct effect	0.0955	0.0940
	residual effect	0.1256	0.1252
Main effect of B	direct effect	0.0961	0.0984
	residual effect	0.1337	0.1268
Interaction AB	direct effect	0.0970	0.0992
	residual effect	0.1307	0.1311

The total variance for the residual effects of design 1 is $0.5882\,\sigma^2$ and for design 2 is $0.6707\,\sigma^2$. Design 2 has been constructed with the minimisation to the main effects of factor A in mind. Both designs 1 and 2 have lower variances for all effects apart from the control effect, either direct or residual, than designs 3.2.1, 3.2.2 and 3.2.3 found in chapter 3 (see table 3.2.1). In general, the designs constructed by the method given in this section have lower variances for all effects apart from the control effect, either direct or residual, than the designs given is chapter 3.

Designs with optimal replication would be recommended in experiments whose primary role is to estimate the main effects of the factors with little regard to the interaction effects. The variances of the control versus other treatments are much higher for these designs than for the ones found in section 3.2 of chapter 3.

Chapter 5
Conclusions

A review of the work in previous chapters is given. Discussion on the various designs for factorial experiments with a control is offered. Further investigations on this topic are suggested.

5.1 *Overview*

The suitability of various change-over designs have been looked at when used in factorial experiments with a control. The theory on the factorial structure for change-over designs for factorial experiments has been shown to be directly applicable for change-over designs for factorial experiments with a control. Designs having the factorial structure simplify the estimation of the various effects of a factorial experiment with a control. Most of the designs given have the factorial structure.

5.1.1 *Optimal designs*

Section 3.6 shows that the designs for t treatments that are universally optimal for the estimation of direct effects as well as residual effects are also optimal for any given set of $t-1$ orthonormal treatment contrasts. Williams squares designs and strongly balanced uniform designs are examples of designs which are universally optimal for the estimation of direct effects as well as residual effects. Designs described as extra-period designs in section 3.6.1 and the designs in section 4.3 also have this kind of optimality. For a factorial experiment with a control in which all contrasts are of equal importance, these designs are the most appropriate. Strongly balanced uniform designs are too large for practical use. Extra-period designs have the short-coming of not having the factorial structure.

When the number of treatments t is odd, consideration is given to an experiment with t periods and t experimental units. Designs given in section 3.4.2 and also those offered by Russell (1991) provide the best option amongst designs generated cyclically, for the estimation of the main effects as well as residual effects for this kind of experiment. These designs would likewise be the best option for factorial experiments with a control treatment when all t-1 orthonormal contrasts are of equal importance.

5.1.2 *Main effects are of greatest importance*

In factorial experiments with a control, situations do arise in which the main interest of the experimenter is to find estimates of the main effects of a certain factor. Chapter 3 provides evidence that designs in which each level of a given factor follows itself and all other levels of the given factor at nearly equal numbers of times give better estimates of the main effects of the given factor than designs in which the frequencies at which the levels of the given factor follow each other are farther from being equal. Designs given in section 3.4.1 give marginally better estimates of factor A than Williams squares designs and are offered as an alternative when the main effects of factor A are of most importance. The gains made by the designs in section 3.4.1 over the Williams square designs are very small so, in the absence of this manuscript, the Williams square designs are highly recommended for such kind of experiments.

Designs having replications which favour the treatment contrasts of interest are a better option. Section 4.4 gives optimal replication for experiments in which the main effects are of greatest importance. The optimal replication was found by using Jones and Eccleston's (1980) algorithms for row-column designs. These designs give better estimates of the main effects than designs given in section 3.4.1. Designs in section 3.4.1 have the factorial structure in their favour while optimal replication designs do not.

5.1.3 *Designs for $p < t$*

Section 4.2 gives designs with the lowest variances for the estimation of the main effects of factor *A* and also for the estimation of the direct effects, amongst all the cyclically generated designs. Jones and Eccleston's algorithms for optimal replication gave equal replication for all treatments for these designs for all kinds of contrasts of interest. Designs in section 4.3 provide universally optimal designs for experiments involving t treatments, $2t$ experimental units and $(t+1)/2$ periods.

5.2 *Further study*

The designs that have been given as alternatives to Williams square designs do not offer much improvements over them. Therefore, almost all research issues which are relevant to change-over designs with unstructured treatment sets are relevant to factorials with added controls.

This paragraph and the next are due to the helpful suggestions of a referee.
The case in which the number of periods (p) is less than the number of treatments (t), introduced in section 4.2, is very relevant to sensory evaluation of food products. If the number of treatments t is small (say 5, 7 or 9), the number of experimental units (judges or panellists in sensory evaluation work) available may exceed t or $2t$. So more $p \times t$ arrays can be appended to the designs given. How best to do this is a topic for further research. Unequal number of periods may also occur in sensory evaluation. Investigation needs to be done on designs with unequal number of periods.

Pigeon and Raghavarao (1987) have provided designs in which the comparison of the control treatment to each test treatment is given more emphasis than comparisons between the test treatments. The designs are said to be control balanced designs. Investigations should be carried out on control-balanced designs in which the test treatments have a factorial nature.

By the same token, the possibility of the adaptation of this approach to the "control unbalanced" situation (see section 4.4 in chapter 4) in which comparison of the control to each test treatment is to be given less emphasis than the comparisons between test treatments.

The algorithms used to determine optimal replication were structured for row-column designs. Further research needs to be done to devise ways of getting optimal replication for change-over designs for factorial experiments with a control .

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